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# OBSERVATIONS
ON A
# SERIES
OF
# ELECTRICAL EXPERIMENTS.

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By Dr. *HOADLY*, and Mr. *WILSON*,
Fellows of the ROYAL SOCIETY.

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The SECOND EDITION.

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With ALTERATIONS and the ADDITION of ſome EXPERIMENTS,
LETTERS, and EXPLANATORY NOTES. By B. WILSON.*

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*Uſque adeo magni refert cui quæ adjaceat res.*
LUCRET. Lib. V.

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BRITISH MUSEUM

# ADVERTISEMENT.

THE former edition of theſe obſervations upon electricity, was publiſhed in conjunction with my late worthy friend Doctor Hoadly; by his death, the care of this ſecond edition devolves wholly on myſelf. It is therefore neceſſary to obſerve, that I have thought it expedient to make ſome alterations, and to add ſome new experiments. I have been mindful, however, to give my reaſons all along for doing this; and yet I am far from imagining that what I even now offer is perfect. The variety of effects which this part of philoſophy affords us, and the *nicety* required in making and obſerving the experiments, is ſo *great*, that I am ſenſible of the neceſſity of ſtill farther improvements before the *data* can be depended upon for ſettling its principles.

*Feb.* 12, 1759.

B. W.

SIR *Iſaac Newton* has ſuppoſed that there is a fluid of the ſame nature with Air, but extremely more ſubtile and elaſtic, every where diſperſed through all ſpace.

This fluid he calls Æther, and ſuppoſes it to be much rarer within the denſe bodies of the Sun, *Stars*, Planets and Comets, than in the empty celeſtial ſpace between them; and in paſſing from them to great diſtances, it grows denſer and denſer perpetually, and thereby cauſes the gravity of thoſe bodies towards one another, and of their parts towards the bodies; every body endeavouring to go from the *denſer* parts of the æther towards the *rarer*.

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[*] This alteration in the beginning is chiefly to do Juſtice to Sir *Iſaac Newton*; as in the former edition he was made to aſſert, rather than ſuppoſe, the exiſtence of a fluid, but which in the following ſheets we ſhall endeavour to make probable.

The earth, therefore, is ſurrounded every where by this æther to a very great diſtance, in conſequence of which the air and all bodies in it gravitate towards the earth, and towards each other, agreeably to the appearances at the ſurface of it.

This æther likewiſe pervades the pores of all bodies, and lies hid in them: and whilſt the bodies with this fluid in them are left to themſelves, (undiſturbed by any external violence) this fluid from its elaſtic nature conforms itſelf, as to its degree of denſity, to the particular make of that body it is in, *e. gr.* It is not ſo denſe in denſe bodies, as in rare ones.

Whence it ſeems to follow, that every body we have it in our power to make any experiment upon, has naturally within it (before it is diſturbed by our experiment) one certain quantity of this fluid, in ſuch a ſtate of rarity or denſity, as is moſt agreeable to the nature of each particular body.

And hence it ſeems reaſonable to conclude, that there will naturally ariſe ſome reſiſtance to every endeavour, that is made, any how to alter the degree of denſity in the whole of any body, or in any particular part of it.

And hence, that it will require ſome degree of force to alter the natural quantity of this fluid contained in every particular body; and more or leſs force according to the nature, and make of each.

Now, as it is univerſally agreed among thoſe, who are moſt converſant with electrical experiments, that the appearances, which occur in thoſe experiments, ariſe from the force and action of a fluid of the ſame elaſtic nature, communicating, and freely paſſing in and out at the ſurface of the earth, and pervading likewiſe the pores of bodies: and as the cleareſt definition of what we mean, when we ſay a body is electrified, is this, that either the body has by the force of the experiment made in order to electrify it, been forced to part with a ſhare of this electrical fluid, that naturally belonged to it during the experiment, and to remain without it ſome-time after the experiment is over: or to admit more than it naturally had within it, during the experiment, and to remain ſo overloaded, ſome time after the experiment is over: *it will be worth our while to enquire whether this electrical fluid, and the æther, be not one and the ſame fluid.*

In order to be ſatisfied in this point, let us ſee in what manner different bodies are thus obliged, on being electrified, either to part with ſome of this fluid, or to receive more of it.

Now from a very great variety of experiments, we have evident proof given, that there is a reſiſtance made by all bodies againſt the admiſſion of any more of this electrical fluid into them, than what naturally belonged to them.

2° That there is a reſiſtance likewiſe made againſt any of this electrical fluid's getting out of all bodies, and conſequently to any diminution of their natural quantity.

3° That this reſiſtance is greater, and leſs in different bodies.

4° That there is a limit, beyond which we cannot encreaſe or diminiſh the natural quantity of this electrical fluid, in each particular body.

5° That when we have thus changed the natural ſtate of this fluid within any body, whether by encreaſing or diminiſhing its quantity, or any other way; there is a reſiſtance, greater or leſs according to various circumſtances, made to the fluid's returning to it's natural ſtate again within that body.

6° and laſtly, That there muſt therefore be ſome accidental or deſigned aſſiſtance given from without (independent on the body and the electrical fluid contained within it) before they can return to their natural ſtate again.

With regard to the different reſiſtances which are made by different bodies, againſt being electrified, or unelectrified; from a great variety of experiments, very carefully made, we may depend upon the truth of the following obſervations, *viz.*

1° That Glaſs, Wax, Roſin, Brimſtone, and ſuch like bodies, reſiſt the moſt, provided they are of a ſufficient thickneſs: and Silk, Hair, *&c.* provided they are of a ſufficient length.

2° That next to theſe ſort of bodies the Air reſiſts moſt, provided there be a ſufficient quantity of it, and it be clear and free from vapours.

3° That this reſiſtance is weakeſt in Metals, Minerals, Water, Quickſilver, Animals and Vegetables, and at the ſurface of the Earth.

But, laſtly, That the reſiſtance in theſe laſt mentioned bodies is greater, when their ſurfaces are poliſhed and extended in length, and the electrifying power acts on the middle of theſe ſurfaces: and leſs, when their ſurfaces are rough and ſhort or end with ſharp points or edges, and the electrifying power acts at thoſe ends.

Theſe facts, I believe, are ſo well known at preſent, that it ſeems unneceſſary to repeat any of the experiments from whence they were deduced. And I ſhall only obſerve, that I have ranged Animals, Minerals, Metals, Vegetables, *&c.* in one claſs, becauſe there is ſo very little difference in their reſiſtance, that it is not worth notice.

Now from this variety in the reſiſtances made by different bodies to being electrified, as well as to their returning again to their natural ſtates; there ſeems to be ſufficient direction afforded us, how to diſpoſe the body we want to try any experiment upon, in the manner moſt ſuitable to the end, we propoſe in making the experiment.

For example, Have I a mind to electrify a bar of iron ſo that it ſhall, receive an additional quantity of the electric fluid, and make a very great reſiſtance to being unelectrified, or to returning again to its natural ſtate; I conſider that ſilk lines of a ſufficient length, kept clean and dry, reſiſt being electrified very ſtrongly; and that air likewiſe, when it is dry and free from vapour, does the ſame: and therefore that a bar of iron ſuſpended carefully by ſilk lines, ſurrounded by air at a proper diſtance from other bodies, is diſpoſed of in the beſt manner to remain electrified ſtrongly, after it is once electrified.

And the reaſon of this is, that the fluid within the bar cannot return to its natural ſtate without part of it is thrown out of the bar; but the ſilk lines, by which it is ſuſpended, and the clean dry air, with which it is every where ſurrounded, reſiſt the admiſſion of this fluid within them the ſtrongeſt of moſt bodies: and therefore when the bar is once electrified, it is thus diſpoſed in the propereſt manner to remain ſo, as all the bodies contiguous to it, will not admit any of this fluid into them, but with the greateſt difficulty.

In the next place, I conſider that a more extended ſurface reſiſts more than one leſs ſo, and that bodies ending with points hardly reſiſt at all; and conſequently, that I ſhould chooſe a bar of iron extended in length, and having its ends ſhaped into ſpherical forms, or ending with large knobs.

And laſtly, I conſider, that if I take care to have a high poliſh given to the bar, I ſhall ſtill give a greater power, when once it is electrified, to reſiſt being unelectrified: which I have ſuppoſed to be the drift of my experiment.

But now I have thus made choice of the moſt proper bar, and diſpoſed of this bar in the beſt manner in order to produce the greateſt effect when it is made to return to its natural ſtate; I have evidently placed it in the moſt diſadvantageous circumſtances for electrifying it: for the ſilk lines, and the quantity of air ſurrounding it, and the particular ſhape, and poliſh of the bar, are all of them nearly equal impediments to any of this fluid's forcing itſelf into the bar, as they are to its forcing itſelf out.

But the ſame way of arguing will lead us to the eaſieſt way of electrifying it, in theſe diſfavourable circumſtances.

Firſt, I conſider that a ſurface but little extended reſiſts leſs, than one more ſo; and therefore, that I ſhall more readily electrify this bar by taking off the reſiſtance ariſing from the air from a ſmall part of the ſurface, than from a larger one.

Secondly, That as metals, eſpecially when they end in points, reſiſt electrifying very little, and conſequently part with the electrical fluid moſt eaſily; I have reaſon to conclude, that if I hang on to the bar a ſmall metal wire doubled, with its two ends ſharpen'd, and reaching to the electrical machine ſo as to have thoſe ends in contact with it, as the part where it is doubled is in contact with the bar, I ſhall on putting the machine in motion moſt readily electrify the bar; and upon removing this wire, when the bar is electrified, I ſhall leave it in the beſt ſtate to reſiſt being unelectrified again.

For this wire, with ſharpen'd points, reſiſting vaſtly leſs than the air that ſurrounds it, very readily admits the electrical fluid flowing into it from the machine, and conducts it to the bar, with which it is in contact, and electrifies it; and when the wire is withdrawn, the air cloſes over the bar, and ſerves to keep it electrified.

Now the ſame way of reaſoning will ſhow us the eaſieſt way of unelectrifying it again.

For firſt, I can take off the reſiſtance of the air by approaching any body more capable, than the air, of receiving the electrical fluid from the bar, nearer and nearer to it, even till it comes into contact with it.

Next I am to conſider what ſhape I ought to give this body. If I make it end with a ſharp point, it will with the greateſt eaſe receive the fluid that endeavours to get out of the bar, as ſoon as the equilibrium begins to be deſtroyed by thus removing the reſiſtance of the air from off that point of the bar, the point of the body is oppoſed to: and if I ſtand upon the ground, whilſt I do this, I give a free paſſage to any quantity of this fluid the bar may have to part with, into the earth.

But laſtly, I am to conſider, that by thus making the diſcharge of this fluid ſo eaſy, it will begin to be made very early in the approach of this pointed body, and the whole be done gradually, and without that violence our deſign was to produce: becauſe as this pointed body approaches the bar, there lies ſo ſmall a quantity of air between the point and the bar, that it hardly reſiſts at all.

Conſequently, we ſhould try the experiment with ſome body that ends bluntly and is well poliſhed, and bring it at laſt with ſome degree of quickneſs within the ſphere of the bar's action to reſtore itſelf to its natural ſtate, in order that it may be forced to do it at once, and not gradually; violently, and not with eaſe.

And the effects produced anſwer in all experiments of this kind, which have been made thus either by chance or with deſign. For in this way the greateſt ſpark has appeared, the loudeſt ſnap has been heard, and the ſtrongeſt feel has been perceived, when the finger's end or knuckle has been made uſe of to make the diſcharge.

How theſe effects are produced on the paſſing of this fluid ſo forcibly from the bar through the air to the body brought to it, it is not my purpoſe at preſent to enquire into: and I ſhall therefore only obſerve here, that as this manner of reaſoning *a priori* is ſo far confirmed by experiment, we have ſufficient encouragement to purſue it farther.

My deſign therefore is to ſee how far the attending to the variety of theſe reſiſtances at the ſurfaces of bodies will ſerve us to explain the particular appearances in every particular experiment, *i. e.* why in ſuch circumſtances this fluid is forced into a body, and retained there; and why in other circumſtances part of it is thrown out, and cannot get in again; laſtly, in what circumſtances the electrical fluid reſtores itſelf to its natural ſtate; and when it will do this at once, and violently; and when gradually, and with eaſe.

In the firſt caſe, when the body has more of this fluid forced into it than it has naturally, it is ſaid to be electrified *plus*; and in the other caſe, when part of its natural quantity is driven out of it, it is ſaid to be electrified *minus*: and in both of theſe ſtates the body ſhews the uſual ſigns of being electrified.

I ſhall begin with the experiments in which bodies are electrified *plus* and *minus*.

Let us now take a bar, that is tapered down at each end to pretty ſharp points; and placing it horizontally on a tall drinking glaſs that is clean and dry; ſee what effects will follow on bringing an excited tube within the diſtance of about ſix inches from the bar (more or leſs, according to the degree of force given to the excited tube) firſt towards the middle of it, and next towards either of its ends. *

In the firſt caſe, the bar will be electrified *minus*, and in the latter caſe *plus*; or in other words, in the firſt caſe, the bar, when the experiment is over, will have leſs of this electrical fluid in it than naturally belonged to it, and in the latter caſe more.

We ſhall begin our explanation of theſe experiments with ſome obſervations on the firſt, where the bar is found to be electrified *minus*.

How we know this will be ſeen by and by.

Now if the bar is electrified *minus*, then ſome of this fluid, more than was forced into it, muſt have eſcaped out of it.

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* See Mr. *John Carton's* experiments in the Phil. Tranſ. Vol. 48, page 350.

The queſtion then ariſes, at what parts of the bar did this fluid eſcape? and the anſwer, according to our manner of reaſoning is very obvious, *viz.* at the tapering ends of the bar, which we have obſerved to reſiſt the exit of this fluid through them, leſs than the extended oppoſite ſurface of the bar.

To be ſatisfied in this point, bring a ſecond bar, ſhaped as the firſt was, into contact with the firſt, end to end, and it will follow from our way of reaſoning, that this ſecond bar will, on repeating the experiment, more readily receive what fluid is thrown out of the firſt, than the air did before; and conſequently, that the fluid, which had before been thrown into the air, will now flow into the ſecond bar.

And accordingly, when the firſt and ſecond bar are placed on two drinking glaſſes, ſo as to make one horizontal line, touching one another at their ends; if an excited tube be brought, as before, to the middle of the firſt bar, the conſequence will be, that the firſt will be electrified *minus* and the ſecond *plus.*

To prove this; before the excited tube is withdrawn, ſeparate the two bars by moving the glaſs which ſupports the ſecond bar: then withdraw the tube, and you will find that on approaching your knuckle to them, they will each of them give the uſual ſigns of being electrified.

But if the experiment be repeated again, without thus ſeparating them; and on withdrawing the excited tube they be left for ever ſo ſhort a time to themſelves, neither of them will ſhew any marks of being electrified on the approach of the knuckle: becauſe they will each of them have returned to their natural ſtate, the ſecond bar having diſcharged its overplus of this fluid into the firſt, which wanted exactly as much to make up its deficiency.

This may be proved to the ſight, by repeating the experiment thus. Bring the tube, as before, to the middle of the firſt bar, and before the tube is withdrawn, ſeparate them: and when the tube is withdrawn, bring them into contact again. The event will be this: A ſpark will be ſeen juſt before they come into contact; and afterwards they will give no ſigns of their being either of them electrified on the approach of the knuckle, for the reaſon already aſſigned.

A plain demonſtration this that one is electrified *minus* and the other *plus*, for it is very well known to be fact, that if both had been electrified *plus*, or both *minus*, no ſuch ſpark would have appeared on their coming into contact, and they would afterwards have given marks of being electrified on the approach of a knuckle towards either of them. The reaſon of this will appear preſently.

And as it appeared above that when the excited tube was applied to the end of the bar, it electrified it *plus*; it is reaſonable to conclude that it is the ſecond bar that is electrified *plus*, as the fluid is forced into it through its end; and conſequently, that the firſt is electrified *minus*, as it was when no ſecond bar was made uſe of: and that the only difference in the two experiments is this, that the fluid is now thrown into the ſecond bar, which before was thrown into the air.

We are next to conſider what happens at the other end of the bar, when no ſecond bar was uſed. Now if the two ends be ſuppoſed to be equally tapered down to points, their reſiſtance will be equally weak, and the elaſtic fluid in the bar being put into a ſtate of dilatation near its middle, will equally preſs both ways to condenſe or throw out the fluid, that ſtands the neareſt to each of its ends.

To try this, we may vary the experiment by applying in the ſame manner a third bar, ſhaped like the others, to the other end of the firſt, ſo that they may all three lie in one horizontal line; and bringing the excited tube, as before, to the middle of the firſt bar, now the middlemoſt bar.

The event will be this: the middlemoſt, or firſt bar, will be electrified *minus*, and the two end ones *plus*.

This may be proved thus. As ſoon as the excited tube has produced its effect, ſeparate the three bars by moving the two outer glaſſes, and they will each ſeparately ſhew ſigns of being electrified on the approach of the knuckle.

Try the experiment over again, ſeparate them as before: but now bring the two outer bars, by moving their glaſſes, into contact at their ends; and no ſpark will appear in doing this, or at leaſt a very inſignificant one, when they happen not to have been exactly electrified equally.

After this, bring firſt one and then the other of theſe outward bars in the ſame manner into contact with the firſt, and in doing this there will appear ſparks, ſufficient to give ſtrong proof of their being electrified: and yet after this, if you approach your knuckle to any of them, no ſigns will appear of their being then electrified; thoſe that had the overplus having readily ſupplied the want of this fluid in the other, and the three bars having in conſequence of this returned to their natural ſtate.

Hence we may conclude, that the quantity of this fluid which had been thrown into the ſecond bar, when only two were uſed, was divided exactly between the ſecond, and third bar in the preſent experiment; and therefore in the former experiment, the fluid that was driven out of the firſt bar paſſed into the ſecond, and little or none was forced out at the other end into the air.

This is the natural conſequence of an elaſtic fluid's dilating itſelf, when there is a reſiſtance to its doing it one way, and little or none another way; for it will moſt certainly flow that way, where the reſiſtance is leaſt.

We may, from what we have ſeen, be pretty well aſſured that if we cover theſe tapering ends of a ſingle bar with hollow pieces of glaſs that exactly fit them, we ſhall in all probability prevent the fluid from paſſing out through theſe ends; and accordingly the experiment anſwers. For when the excited tube is brought to the ſame diſtance from the middle of the bar, as before;

the bar, now its end are thus fortified with glaſs, will be electrified *plus*, inſtead of *minus*, even though the tube be withdrawn: which ſhews how much more ſtrongly the extended ſurface of the bar, oppoſite to the excited tube, reſiſts the fluid's eſcape, than the tapered ends did.

So much (at preſent) for the firſt experiment, in which the tube was preſented at a diſtance to the middle of a ſingle bar, and electrifies it *minus*.

In the ſecond experiment, when the tube was preſented at the ſame diſtance to the end of the bar, it was electrified *plus*, and therefore more of this fluid was thrown into the bar at one end, than was ſuffered to paſs out at the other: which was reaſonably to be expected, as the tube, acting with no more force than it did before, is now at double the diſtance from the end, at which the fluid is to eſcape; and therefore has a greater reſiſtance to overcome before any can be thrown out at that end.

But yet, that ſome of this fluid does eſcape out at the oppoſite end, will appear from bringing a ſecond bar into contact, end to end, with the firſt; for then, on repeating the experiment, they will both be electrified *plus*: and on bringing a third in the ſame manner, end to end with the ſecond, they will all three be electrified *plus*.

But then the third bar will be electrified *plus* leſs forcibly than the ſecond, and the ſecond leſs than the firſt.

By which it ſhould ſeem, that if a fourth bar, and a fifth, and a ſixth, &c. were in the ſame manner added to the ſtring of bars; the moſt diſtant bar, from the excited tube, would at laſt not be ſenſibly affected at all by it; and conſequently, that the virtue of the tube is limited, and can affect the fluid in theſe bars to a certain diſtance only.

But this will be made more evident hereafter.

Let us now reflect a little on what we have ſeen with regard to the air, ſurrounding the bars in theſe experiments.

Firſt, We have ſeen that although air makes a ſtrong reſiſtance againſt being electrified, *i. e.* againſt admitting more of this fluid into it than what naturally belongs to it; yet the excited tube does overcome this reſiſtance, and forces the fluid within the bar through its ends into the air, in the experiments with a ſingle bar, both when it was electrified *minus* and *plus*.

2° That the quantity of this fluid, forced thus into the air in the firſt experiment, was ſufficient to electrify the two bars *plus*, that were applied to its ends on repeating that experiment.

3° That none of this fluid was diſſipated in the air, and ſo loſt, becauſe on withdrawing the excited tube, and leaving the three bars to themſelves, they ſhew no ſigns of being electrified at all.

It ſhould ſeem therefore, that when a ſingle bar is thus electrified *minus*; the fluid thrown out of it into the air ſtands in that portion of air that ſurrounds the ends of the bar all the time the excited tube continues acting; and that, when the tube is withdrawn, it returns again into the bar, till ſuch time as the reſiſtance at the ends is a ballance to the force, with which it endeavours to flow in. But as the bar remains electrified *minus* after the experiment is over, it is evident that all that was thrown out does not return in again; and therefore that the remainder forms itſelf into a kind of atmoſphere every way ſurrounding the bar with a nearly equal degree of denſity, from the nature of the two elaſtic fluids, air, and the electrical fluid, and of the reſiſtance that fluid meets with in its endeavour to dilate itſelf either into the air, or into the bar.

4° We have likewiſe ſeen, that when a ſingle bar was electrified *plus*, there was likewiſe a ſufficient quantity of this fluid to electrify two bars applied to its other end.

It follows therefore, that when a bar is electrified *plus* by itſelf, and continues ſo after the tube is withdrawn, the fluid that was thrown out of the bar muſt remain there, and form the ſame kind of atmoſphere round the bar, as it does in the other caſe, when it was electrified *minus.*

Whenever therefore a Body is electrified either *plus*, or *minus*, and remains ſo after the experiment is over, there are ſimilar atmoſpheres of the electrical fluid ſurrounding them, that are ready to expand themſelves into any body that approaches, that reſiſts leſs than the air: and this is the reaſon why bodies give very nearly the ſame ſigns, when they are electrified either *plus* or *minus*.

I ſay, very nearly, becauſe when we come to examine the ſigns theſe bodies give (on being electrified in theſe different ways) with greater accuracy, we ſhall ſee a ſufficient difference in theſe ſigns to enable us to ſay which were electrified *plus* and which *minus*.

It will be worth while to ſtop here a little, and conſider the different circumſtances the electrical fluid is in, that forms theſe ſimilar atmoſpheres around a body electrified *plus*, and one electrified *minus*.

When a bar is electrified *plus*, the * atmoſphere formed round it is extended in the air to a conſiderable diſtance; but the air reſiſts its entrance into it more forcibly than the bar naturally does: and the bar, by being overloaded by electrification, reſiſts its return into it, (even when the excited tube is withdrawn) more forcibly than it does naturally; and by being thus circumſtanced, its endeavour to expand is naturally exerted outwards from the axis of the bar on every ſide, and it gradually diſſipates itſelf into the air, and whilſt it is doing this, and no longer, the bar will remain electrified *plus*.

When a bar is electrified *minus*, the atmoſphere formed round it, which during the action of the tube that electrified it, ** is extended in the ſame manner in the air to a conſiderable diſtance from the bar, will, on that tube's being withdrawn, exert its endeavour to dilate itſelf in a contrary direction, *viz.* from the ſurrounding air on every ſide inwards to the axis of the bar; and it will gradually flow into the bar, and reduce the electrical fluid there to its natural degree of denſity; and whilſt this is doing, and no longer, will the bar remain electrified *minus*.

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* The electric atmoſphere is not to be underſtood as lying between the air and the bar, as it is expreſt in the former edition, but extended in the air all the way from the bar.

** This alteration was in conſequence of the former.

When therefore two balls are both of them electrified *plus*, ſuſpended by two ſilk lines, and brought near one another; they repel each other, and ſtand for ſome time at a diſtance from each other; becauſe the two atmoſpheres, each of them exerting their endeavours to expand into the air, want more room to do it in; and when the weight of the balls is not ſufficient to prevent it, muſt naturally drive them aſunder, till theſe atmoſpheres are diſſipated, and the weight of the balls takes place again.

And when two bodies are both of them electrified *minus*, ſuſpended by ſilk ſtrings, and brought near one another, they likewiſe repel each other, and ſtand for ſome time at a diſtance from each other; becauſe the condenſed electrical fluid in the air, in order to force itſelf in at the ſurfaces of the balls between their two centers, crouds in, and forces them aſunder, till the atmoſpheres get all into the balls, and their weight then takes place again.

But when two balls are in the ſame circumſtances, one electrified *plus*, and the other *minus*, and brought near one another; they will gradually come together and unelectrify each other. Becauſe the atmoſphere of the ball electrified *plus*, is endeavouring to diſſipate itſelf from the center of the ball outwards; and the atmoſphere of the ball electrified *minus*, is endeavouring to dilate itſelf from the air inwards to the center of the ball. The common atmoſpheres therefore of the two balls, thus brought near together, exert their forces in one and the ſame direction between them; the flow of the electrical fluid into the ball electrified *minus*, is facilitated by the endeavour of the electrical fluid to get out of the ball, electrified *plus*; and *vice verſa*: and ſo the two balls and the air between them very readily return to their natural ſtates. *

Thus then it appears, that when we only know that a body is electrified, without knowing in what manner it was electrified, there is no criterion to form our judgment upon whether it was electrified *plus*, or *minus*: becauſe the common appearances are the ſame in both cases, when we unelectrify it.

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* The reader is deſired to take notice, that though two electrical atmoſpheres ſeem neceſſary in this experiment to reſtore the equilibrium in the balls, yet he is not to conclude therefore that they both enter the balls, becauſe the electrical ſigns of both diſappear at the ſame time. But this ſeems to follow from the principles we have already deduced, that a quantity, equal to what was in the ball electrified *plus*, is at the end of the experiment diffuſed in the balls electrified *minus*; and the remaining quantity which form'd the atmoſpheres is diffuſed in the air, or the bodies from whence it at firſt flow'd, in order to crowd into the body electrified *minus*. For if this was not the caſe ſuch bodies, or the air, would remain in an unnatural ſtate, becauſe the equilibrium of the electric fluid in ſuch circumſtances would not be reſtored.

But by purſuing the ſame train of reaſoning, we ſhall readily obtain a certain method to know whether a body is electrified *plus*, or *minus*, even without unelectrifying it.

Faſten two cork balls one at each end to a piece of thread about eight inches long, and, doubling the thread over the bar before it is electrified, make the balls hang as near to one another under the bar, as they can; and now the bar, the threads, and the balls, ſhould be conſidered as one body equally ready to be electrified either *plus*, or *minus*.

1° We will ſuppoſe this bar, *&c.* to be electrified *minus*, and in conſequence of their being electrified at all, the balls to repel one another, and hang at a greater diſtance from each other, than they did naturally.

Now let an excited tube be brought to a certain diſtance under theſe balls in theſe circumſtances, and they will at firſt repel each other more; becauſe the force of the excited tube will condenſe the atmoſpheres around the balls ſtill more, on account of the reſiſtance of the air, till the reſiſtance at their ſurfaces is overcome, which will take up ſome little time, during which, their atmoſpheres being encreaſing, they will repel each other more than before the tube was brought near them.

But ſo ſoon as ever this reſiſtance is once overcome, the excited tube drives the atmoſpheres into the balls, *&c.* and conſequently begins to unelectrify them, *i. e.* to render them leſs forcibly electrified *minus*: and on withdrawing the tube, the balls will hang very viſibly nearer together than they did before the tube was firſt brought near them.

2° We will ſuppoſe the bar, *&c.* to be electrified *plus*, and in conſequence of their being electrified at all, the balls to repel each other.

Now when an excited tube is brought to a certain diſtance under the balls in theſe circumſtances, they will repel each other leſs forcibly and come nearer together; becauſe the reſiſtance of the air alone to the electrical fluid's eſcaping out of the balls electrified *plus*, has been ſeen not to be able entirely to prevent it, as an atmoſphere is made, and ſupported round the balls, till they are reduced to their natural ſtate. But now the excited tube acts in concert with the air, this atmoſphere, which had prevailed againſt the air alone, muſt, on this additional force acting againſt it, retire again into the ball, and continue to do ſo for ſome ſmall time.

And after it is all retired into the balls again, ſo long as the bar, *&c.* can be electrified more and more *plus*, this appearance will continue, whilſt the tube remains in action: and when the tube is withdrawn, the balls will repel each other more forcibly than at firſt, becauſe they will remain more forcibly electrified, *plus*.

Thus then it appears that though the balls repel each other when the bar is electrified either *plus*, or *minus*, yet when an excited tube is brought near them in this their repulſive ſtate, they will in one caſe have their repulſive force encreaſed at firſt, but very ſoon after gradually diminiſhed: and in the other caſe, diminiſhed at firſt, but very ſoon after encreaſed.

And when we ſee the appearance in the firſt caſe, we may ſafely conclude the bar had been electrified *minus*; and when we ſee the appearance in the other caſe, we may conclude the bar had been electrified *plus*.

This, therefore, is an eaſy and ready way of trying, when there is any doubt about it, whether any body is electrified *plus*, or *minus*. For the ſtring, with the balls affixed to its ends, may, with the aſſiſtance of a glaſs tube, be eaſily hung on the bar without unelectrifying it, ſuppoſing it to have been electrified at firſt without the ſtring and balls.

I cannot help obſerving here, that in the firſt caſe, after the excited tube has been held ſo long near the balls, that on its being withdrawn they no longer repel each other; if it be again preſented to them, it will electrify them and the bar *plus*, and they will be put into a repulſive ſtate again.

Whence it was reaſonable to conclude, that if the excited tube, in our firſt experiment, after it had been preſented at a proper diſtance to the extended ſurface of the bar, and ſo had electrified it *minus*, had been brought nearer and nearer to the bar, ſo as at laſt to touch it, it would have electrified it *plus*.

And accordingly, when the experiment was tried, it ſucceeded, and the bar was electrified *plus*.

Now the conſequence of this is, that there muſt be ſome middle diſtance of the excited tube from the bar, between its ſituation, where it electrified it *minus*, and its ſituation, where it electrified it *plus*; at which middle diſtance, the bar will be reduced to its natural ſtate, and not be electrified any more, than if the tube was not there.

This would appear a moſt amazing paradox in electricity, if it was told in general terms to any one, who did not know that bodies were capable of being electrified *plus* and *minus*: *viz.* the ſame excited tube brought near a body electrifies it; and after that, (without ever withdrawing it) brought nearer, ceaſes to electrify it; and after that, brought ſtill nearer, electrifies it again.

Now as this was deduced from our way of reaſoning, the event, anſwering on making the experiment, greatly confirms the truth of the reaſoning.

But I have not yet done with the cork balls, as they will be very ſerviceable in proving, that the power of the excited tube (or other electrifying machine) is limited, and can electrify bodies but to a certain degree, at one and the ſame diſtance from thoſe bodies.

This I hinted at before, and promiſed to ſhew more fully: and in order to do it, I ſhall uſe theſe balls as indexes to ſhew us when, and to what degree the bar, to which they are fixed, is electrified: which end they will exactly anſwer, becauſe they will repel each other with more or leſs force, according to their greater or leſs degree of electrification.

Let the balls, therefore, be hung at the end of the bar, and the excited tube be brought to a certain diſtance from the middle of it, and held there ſteady and firm, and let us ſuppoſe that at this diſtance it electrifies the bar *minus*.

Now I ſay, that ſome little time is taken up, during which the tube electrifies the bar *minus* to a certain degree; but that when it has once done this, it will produce no farther effect on it, though it remain at that diſtance ever ſo long, and be ſuppoſed to loſe none of its force.

Now this is ſhewn to be true in fact by trying the experiment thus with the cork balls; for you will ſee, on bringing the excited tube to this diſtance from the bar, the balls will begin to repel each other, and continue to do ſo more and more, till they ſtand at a certain diſtance aſunder, and then they will continue ſo, without ever getting farther aſunder, how long ſoever the tube is held at that diſtance.

This is a proof of my aſſertion, that the Power of the excited tube is limited, and therefore can act but to a certain degree in electrifying bodies, which will be greater or leſs, according to the circumſtances of each particular experiment.

But this will be confirmed by the following very remarkable ſet of experiments.

Let a glaſs tube be hermetically ſealed at one end, and have its other end properly armed with braſs, cemented into it; and let proper contrivances be made in that braſs, ſo that the tube may be readily fixt to the air-pump and exhauſted of air; and afterwards removed from it, and ſtill remain exhauſted.

1° Let this tube be fixt to the air-pump in order to be exhauſted of air.

Now before this is done, the outer and inner ſurface of the glaſs tube are equally expoſed to the air; and conſequently, the powers of theſe two ſurfaces are in equilibrio, even ſuppoſing the air to affect them.

2° Let the tube be exhauſted of air, as perfectly as it can.

When this is done, the outer ſurface remains, as before, expoſed to the air; and the inner ſurface is expoſed to the electrical fluid, which I will ſuppoſe to be naturally diſperſed in empty ſpaces void of all groſs bodies, (as the vacuum is thought very nearly to be) as well as in the pores of groſs bodies.

And the equilibrium is ſtill preſerved between the powers at theſe ſurfaces: as we may conclude from there being no viſible or perceptible alterations obſerved to be produced on drawing the air out of the hollow of the tube, either in the light, or in the dark.

Whence we may conclude that the air, when it has its natural quantity of this electrical fluid only in it, does not affect this tube at its outer ſurface, more than the fluid within it does at its inner ſurface.

3° Let the exhauſted tube be taken off the air-pump: and let a perſon graſp the braſs end of it with his hand, whilſt he ſtands on the ground.

4° Let another perſon bring an excited tube near the outer ſurface of any part of this exhauſted tube (ſuppoſe near the end that is hermetically ſealed, for this reaſon only, that it will then be at a diſtance from the hand that graſps the other end of it.)

There will immediately appear lucid rays of light, very viſible when the experiment is made in the dark, proceeding from the inner ſurface of that part of the tube neareſt the excited tube, and darting through the vacuum to the braſs, graſped by the hand.

5° And if the tubes are held ſteady for ſome length of time, ſo as to be kept at the ſame diſtance from each other; this lucid appearance will ceaſe, and the light totally diſappear.

6° And when the light has thus diſappeared for ſome time; if the excited tube be withdrawn entirely, the rays of light will appear again; but they will now be ſeen darting from the braſs end of the exhauſted tube through the vacuum to the inner ſurface of that part of the tube, from which they had proceeded before: and this darting of the rays from the braſs end of the tube will continue for a time only, as the other had done before.

7° And when the excited tube is brought nearer, and nearer to the exhauſted one, as the light diſappeared, there only appears a freſh darting of rays, ſimilar to the firſt, which likewiſe continues for a time only: and on the removal of the excited tube, the ſame kind of light, ſomewhat ſtronger and in greater quantity, returns from

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the braſs end of the tube, as before, and diſappears again after ſome little time. *

In this experiment there is the ſtrongeſt appearance of the electrical fluid's ſhifting its place in order to preſerve the equilibrium, by flowing immediately, when any force is offered to the outer ſurface, from its inner ſurface through the vacuum, and darting directly to the metal and hand, where it finds the reſiſtance to its eſcape is the weakeſt.

Now though the reſiſtance is weakeſt, where the braſs and hand is applied to the glaſs; yet there is a reſiſtance; and conſequently, the electrical fluid in the vacuum muſt have ſome additional force given to it from that of the inner ſurface of the glaſs which is neareſt to the excited tube, *i.e.* muſt be condenſed till it acquires a power ſufficient to overcome the reſiſtance of the braſs, *&c.*

But this fluid within the exhauſted tube cannot be condenſed, without more paſſes into it immediately from that inner ſurface, and conſequently ſo long as the electrical fluid is continually flowing from it into the vacuum, and ſo into the braſs, *&c.* ſo long will it be loſing its natural quantity, unleſs its loſs of this fluid be ſupplied by a flow of more from the excited tube.

I ſhall now, therefore, endeavour to ſhew, that the loſs of the electrical fluid flowing from the inner ſurface of the glaſs, is not ſupplied by a ſimilar flow of it from the excited tube to it, through the outer ſurface; and therefore, that the glaſs is electrified *minus* at or near that part of its inner ſurface.

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* Mr. *J. Canton* has taken notice of this vaniſhing and returning of the light in the ſame experiment, referred to in page 10.

Now this will follow plainly, if I can ſhew, 1° That no electrical fluid flows into the glaſs ſo far as to reach that inner ſurface; and, 2° How part of the electrical fluid naturally belonging to it can be driven out into the vacuum, without ſuch a flow of the fluid from the excited tube through the glaſs to get at it immediately.

In order to prove the firſt of theſe, the beſt way ſeems to me to be, to ſuppoſe the contrary, that the electrical fluid does flow into it, and ſupply it continually, and to enquire what would be the conſequence of ſuch a ſuppoſition at the end of the experiment.

If upon this enquiry, we find that the appearances anſwer in fact to what we ought to expect from this ſuppoſition, there will be no reaſon to reject it. But on the contrary, if the appearances are againſt it, it muſt be given up.

Now if there was ſuch a flow of this fluid from the excited tube through the whole ſubſtance of the glaſs to the very inner ſurface of it, ſo as to ſupply the very quantity it drives out through it; the glaſs at this inner ſurface muſt always have its natural quantity in it; and conſequently, when the excited tube is withdrawn, remain in its natural ſtate, equally capable, as at firſt, to reſiſt the entrance of any more of this fluid into it.

And therefore, on withdrawing the excited tube, there would be no ſuch return of light from the braſs, &c. darting ſo particularly towards that part of the inner ſurface of the glaſs, as appears in the experiment.

So far then the appearances in the experiment do not anſwer agreably to what we ſhould naturally expect from the ſuppoſition.

Again, if the excited tube ſupplies the inner ſurface of the glaſs with as much of this fluid as paſſes through it into the vacuum; it will follow, that as long as the excited tube has any power, there muſt be a continuation of this lucid appearance in the vacuum; but this is by no means true in fact, that appearance vaniſhing long before the excited tube has loſt its power; as was ſeen above.

Here then is another remarkable inſtance, in which the appearances do not anſwer on the trial to what was reaſonably to be expected from the ſuppoſition.

And therefore we ought to conclude that no electrical fluid paſſes from the excited tube ſo far into the glaſs, as to reach its internal ſurface and ſupply it with any recruit of thoſe particles of this electrical fluid, which are thrown through it into the vacuum: and conſequently, that this inner ſurface remains electrified *minus* during the continuance of the excited tube near the glaſs, after the lucid appearance has ceaſed; and ſtands ready (on withdrawing that tube, or on its power's decaying) to receive a ſupply of what it had loſt from the equilibrium being reſtored through the perſon, his hand, the braſs and the electrical fluid that is in the vacuum.

But that no doubt may remain in an enquiry of ſo delicate a nature, I will propoſe making the experiment in a more accurate manner.

Every one knows that a bar of iron, ſuſpended by ſilk ſtrings in the air, may be kept equally electrified, or very nearly ſo, for any length of time, by the electrifying machine's being kept equally in motion, during that time.

Now if inſtead of approaching an excited tube of glaſs (which is continually loſing its power) to the exhauſted tube, as I did before; I now approach the exhauſted tube to the electrified bar (whoſe electrifying force is conſtantly kept up at the ſame degree by the machine) to ſuch a particular diſtance, as to make the lucid appearance begin; and then keep it there ſteady: I gain this advantage, that it continues during the whole time I keep it thus ſteady at one certain diſtance, expoſed to one and the ſame degree of force in the electrified bar, or very nearly ſo.

And on trying the experiment thus, I found that the lucid appearance continues only for a limited time; and if the tube was to be held ſo ever ſo long, there would be no return of that appearance: but on removing the exhauſted tube from the bar, the light returns again immediately, ſhooting then from the braſs and hand to that part of the inner ſurface of the tube, that had been neareſt to the bar.

No experiment can be more to the purpoſe, than this; and we may now fairly conclude, that the electrical fluid does not flow in ſo plentifully, or ſo forcibly as to reach the inner ſurface of the glaſs, and ſo ſupply it with a quantity equal to what is thrown off through it into the vacuum, on the firſt approach of the excited tube in the firſt way of trying this experiment, or on the firſt approach of the exhauſted tube to the electrified bar, in the laſt way.

It remains therefore to be ſhown how this inner ſurface, and the part of the glaſs neareſt it, can be ſo affected, as it is, without ſo ſtrong a flow of this electrical fluid from the tube or bar.

In order to be more plainly underſtood, I ſhall obſerve, that the electrical fluid within the hollow of the exhauſted tube is in the ſame circumſtances with that in the firſt bar, when a ſecond bar was brought into contact with it end to end: and that this ſecond bar is repreſented in this experiment by the braſs and the perſon holding it. Now as the excited tube, applied to the middle of the firſt bar, dilated the electrical fluid that lay neareſt it with ſufficient force to drive that at a diſtance into the ſecond bar: ſo in this experiment, the excited tube has power enough to dilate the electrical fluid at the inner ſurface of the glaſs near it with ſufficient force to drive the diſtant part of it in the vacuum, into the braſs, and the perſon, who holds it: though it has not power to force any external electrical fluid into the glaſs itſelf.

But this power is limited, and the excited tube can dilate this electrical fluid in theſe circumſtances but to a certain degree; and this it muſt do gradually: and conſequently, during the time of doing this, there will be a flow of this fluid from the inner ſurface of the glaſs to the braſs, *&c.* which manifeſts itſelf by a ſtream of light.

And when the electrical fluid in the braſs, hand, *&c.* is ſufficiently condenſed to ballance the force of the dilating fluid in the vacuum; no more of it will be driven out, the flow of it will ceaſe, and the lucid light will diſappear.

So long therefore (after the light diſappears) as the excited tube is held at the ſame diſtance, and acts with the ſame force; the electrical fluid within the vacuum and the braſs, *&c.* will be kept in equilibrio, the electrical fluid neareſt the excited tube being all that time forced to continue in that degree of dilatation by the continued force of that tube, and ſo to keep the more diſtant part of the fluid near the braſs in the ſame degree of condenſation. For ſurely the ſame power, that is able at firſt to put them into theſe different ſtates, muſt be able to keep them in thoſe ſtates, ſo long as it acts with the ſame force in the ſame circumſtances.

But on withdrawing the tube, the force of the condenſed part of the fluid in the braſs drives that in the vacuum back again to the inner ſurface of the glaſs, where the tube had been applied; and conſequently, there muſt be a returning flow of this fluid from the braſs to the glaſs, which will manifeſt itſelf by a ſimilar appearance of light ſhooting from the braſs to the glaſs; and this will laſt no longer than till the equilibrium is reſtored.

And this effect is ſimilar to what happens in the two bars when they are left to themſelves after the tube is withdrawn, the equilibrium here too being very ſoon reſtored by the condenſed part of the fluid in one bar's gradually flowing into the dilated part in the other.

And conſequently theſe two experiments illuſtrate each other, and the reaſoning thus on them both together is ſufficient to convince us, that the electrical fluid within the ſubſtance of the glaſs at its inner ſurface is forced into a ſtate of dilatation by the power of the excited tube at a diſtance: which it is in this caſe enabled to perform by there being no reſiſtance of air at that ſurface to keep that fluid from dilating.

From theſe experiments we are naturally led to attempt an explanation of the appearances that occur in electrifying and unelectrifying a large pane of glaſs in a ſimilar manner to that in which the *Leyden* bottle was *accidentally electrified* at firſt. Before the appearance, that happened on unelectrifying that bottle: no one imagined that glaſs could be electrified any other way, than by being *actually rubbed*. But experience ſince has taught us otherwiſe, and that in order to electrify glaſs ſo as to produce theſe appearances with conſiderable force, it is neceſſary there ſhould be extended over the two oppoſite ſurfaces of the glaſs equal coverings of metal, or ſome ſuch body as eaſily electrified: but how theſe coverings contribute to that extraordinary effect has not hitherto been ſatisfactorily explained.

We will ſuppoſe then an oblong pane of glaſs to be covered in the middle of its upper ſurface with leaf gold, ſomehow kept in contact with it, of the ſame ſhape with the pane of glaſs, but no where reaching within two, or three inches of its edges: and that the ſame be done on its under ſurface, ſo that the under-covering may lie directly under the upper-covering.

The uſe of the upper-covering is this. As every part of it, from the nature of metal, is equally electrified at the ſame time that any one part of it is; the whole extended ſurface of the glaſs immediately under it lies equally expoſed to the action of the electrical fluid, when that cover is electrified.

Let us then ſuppoſe that a Communication is made by a wire from the electrifying machine to this upper covering: it will follow then, that, whenever the machine is put into motion, the electrical fluid will be directed down the wire into the upper covering, and when there, will exert its full force againſt its whole extent of the glaſs immediately under it: and that this force will be greater than the force of an excited tube on the ſame ſurface of glaſs would be, when it was only brought near it, becauſe the flow of the fluid to the glaſs is leſs reſiſted by the metal wire and covering, than it would be in the other caſe by the air it muſt paſs through to get at it; and this is another uſe of the covering.

Let us in the next place ſuppoſe a communication to be made by another wire between the under covering and the ground, it is plain on a ſuppoſition that the glaſs was electrified, that great part of the reſiſtance to the eſcape of the electrical fluid through the under ſurface of the glaſs will be taken off, as metal reſiſts ſo much leſs than air, and all the whole ſurface of the glaſs immediately over this cover can diſcharge its fluid into the wire at once, and ſo into the ground. This therefore is the uſe of the uncovering and its wire.

Let us next attend to the glaſs. We are to conſider no more of it, as to its being electrified, than what lies immediately between its two coverings; for the rims of the glaſs, that are left uncovered and expoſed to the air, ſerve to prevent any of the electrical fluid's paſſing through the edges or points of the pane of glaſs or from one cover to the other.

Now this parallelopiped of glaſs (when no communication is made with the ground) ſeems to be much in the ſame circumſtances with the bar when its two ends were armed with glaſs; and we ſaw the bar was in thoſe circumſtances electrified ſtrongly *plus* when the excited tube was brought into contact with it, and ſo had no air between it and the bar to reſiſt the entrance of the fluid into the bar.

Though the reſiſtance is not ſo entirely taken off, in the preſent caſe, as if an excited tube was in contact with the glaſs, yet it is greatly diminiſhed by the electrical fluid's being conducted thus to the glaſs through metal, without paſſing through air at all: and therefore we have reaſon to believe the glaſs is put into the moſt favourable circumſtances to be ſtrongly electrified, from every thing we have yet ſeen in electrifying of metals.

But experience has ſhewn us the contrary, and that glaſs cannot be ſo electrified; but that the reſiſtance muſt be taken off from its under, as well as from its upper ſurface. It is therefore contrived, by placing the covers and wires as we have done, that the eſcape of the fluid may be as much promoted through the under cover by the wire that communicates with the ground, as the entrance of it into the glaſs through the upper cover is by the wire that communicates with the machine.

We ſhall now, therefore, propoſe an experiment on the pane of glaſs very nearly in theſe circumſtances.

Let the pane of glaſs, covered thus on each ſurface, be ſupported by four drinking glaſſes, one under each corner; and let a finger be held within an inch of the under covering by a perſon ſtanding on the ground; and let the wire, which communicates with the electrifying machine, reach likewiſe wihin an inch of the upper covering, and end with a knob, as large as the finger's end.

By thus bringing the finger, and the wire almoſt, but not quite into contact with the covers, we gain this advantage, that when any electrical fluid flows from the wire into the upper covering, or from the under covering into the finger, it muſt paſs through an inch of air and ſo give a viſible proof of its doing ſo, by producing a ſpark.

If now the machine be ſet in motion, there will a ſpark be ſeen iſſuing from the wire, and at the ſame time an equal ſpark from the under cover; and then another and another from the wire, accompanied at each of the times with an equal ſpark from the under cover; but theſe ſparks will continue thus ſucceediug each other from the wire and under cover for a certain time only, and then ceaſe, notwithſtanding the machine be kept equally in action for ever ſo long a time, and the wire and finger be kept in the ſame places.

Now from this experiment it ſhould ſeem, that whatever was thrown in at the upper cover paſſed out again immediately in equal quantites at the under one, and therefore, that the electrical fluid has a free paſſage through the pores of glaſs, and meets with no reſiſtance; and conſequently, that the glaſs is not itſelf electrified in theſe circumſtances; and indeed, if after theſe ſparks have ceaſed appearing, and both the wire of communication and the finger are removed; a finger be brought to either covering at different times, there will none or very ſlight ſigns appear of the glaſs's being electrified.

But if any one, who argues from theſe facts that the glaſs is not electrified, will hold one finger in contact with one cover, and then approach a fiinger of his other hand to the oppoſite cover, he will receive ſo ſevere a ſtroke, as will convince him that the glaſs was very ſtrongly electrified; though before it gave little or no ſign of it, in the manner metals do; which part with their overload readily whereſoever they are touched by a body that is not electrified itſelf.

The fact however is undoubtedly true, that as much of this fluid is thrown out of the glaſs at the under ſurface through the under covering into the finger, as is thrown into it from the wire at its upper ſurface through the upper covering; but the reaſoning upon this fact is evidently falſe, as it contradicts experiment.*

Let us ſee how our doctrine of reſiſtances will enable us to ſolve this difficulty.

Let us call the reſiſtance at each ſurface of the glaſs, ſix; by which I mean that the upper reſiſts the entrance of the fluid into the glaſs, and the under reſiſts its exit out of it; with the ſame force, equally to ſix: and that the machine acts with a force equal to nine.

Then will ſix of this nine be ballanced by the reſiſtance, ſix, at the upper ſurface, and the fluid will be forced into the glaſs with the force, three only; and conſequently, when the machine has exerted this force, nine, and condenſed the electrical fluid within the glaſs to ſuch a degree, as the force, three, can condenſe it to; it can do no more; becauſe the reſiſtance at the ſurface, ſix, and the expanſive power of this fluid, three, (which, from the nature of an elaſtic fluid, endeavours, wherever it lies, to expand itſelf equally every way with the ſame force with which it is condenſed) added together makes nine, *i. e.* is an exact ballance to the force of the machine.

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* See Dr. Franklin's letters upon this ſubject.

Now this fluid, thus forced into the glaſs, endeavours immediately to expand itſelf every way, as ſoon as ever it begins to be thrown into it, towards the edge and corners of the glaſs, as well as towards the oppoſite ſurface.

But theſe edges and corners of the glaſs are every way ſo ſtrongly guarded by the length of the rim that is purpoſely left expoſed to the air, that the fluid is prevented to eſcape through them by the reſiſtance of the air and the thickneſs of the glaſs it has to paſs through; whilſt the under ſurface is only ſuppoſed to reſiſt with the force, ſix, and is purpoſely cut off from any aſſiſtance from the air: and conſequently from the time, that any of this fluid is thrown into the glaſs at the upper ſurface till all is thrown in (that the force, three, can throw into it) as much will be thrown out at the under ſurface, where the reſiſtance is leaſt; or in other words, the reſiſtance at this ſurface will gradually be reduced to three, and then there will be an equilibrium between all the forces.

Thus we have ſeen before, in the experiment of the exhauſted tube, that the excited tube, when kept at a certain diſtance from it, had a power to drive the electrical fluid out at the inner ſurface of the glaſs, though no electrical fluid paſſed ſo far into the glaſs as to reach that ſurface; and therefore we may here imagine it poſſible, that the excited fluid penetrating thus to a certain diſtance within the upper ſurface may act at a diſtance on that near the under ſurface, and ſo force it out, till there is a ballance between the expanſive force of this condenſed fluid, and the reſiſtance at the under ſurface: and thus the reſiſtance ſix at the under ſurface, will be reduced to three; juſt as the force, nine of the machine was reduced to three, by the reſiſtance, ſix, at the upper ſurface of the glaſs: and all will remain in equilibrio.

Conſequently, all the while the machine is condenſing the fluid within the glaſs near its upper ſurface, it is likewiſe dilating the fluid, naturally in the glaſs, near the under ſurface; which it cannot do without forcing ſome of it out: and therefore it is reaſonable to expect from this doctrine, as the wire and finger are not in abſolute contact with the covers, that ſparks of fire ſhould continue to appear at both ſurfaces during this time, and no longer; and that the inſtant they diſappear, the fluid within the glaſs exactly ballances the fluid without.

For the machine, never acting with more force than nine, has this force ballanced by ſix, the reſiſtance at the upper ſurface, with the additional force, three, of the condenſed fluid near it; and the condenſed fluid acting the contrary way towards the under ſurface with the ſame force, three, is ballanced by the remaining force, three, at that under ſurface.

Now the only difficulty here ariſes from our want of conceiving how a quantity of elaſtic fluid can lie condenſed in one part of the glaſs, whilſt its neighbouring fluid in another part is attenuated; but when experiment is ſtrongly on one ſide, and nothing but this difficulty of conception appears on the other, the latter ought to give way: however, that we may not reſt to much upon this, we will purſue our enquiries farther, and try to remove this difficulty.

To explain how experiment is ſtrongly on one ſide, we muſt conſider the thing more carefully, and compare the preſent experiment with that of the exhauſted tube.

In the experiment of the exhauſted tube, we ſaw that the excited tube acted at a conſiderable diſtance on the glaſs of the exhauſted tube, ſo as to produce a lucid appearance in the vacuum for a time; and on withdrawing the excited tube the lucid appearance returned again, but in a contrary direction.

Let us now repeat our preſent experiment, and try what will be the event, if we withdraw the wire of communication with the machine, as ſoon as the appearance of ſparks ceaſes; keeping the finger ſtill at the ſame diſtance from the under cover.

The event is this. No return of ſparks is perceived.

This therefore ſeems to be an experimental Proof, that there is ſome power remaining *in the glaſs itſelf* * that prevents the return of theſe ſparks, or rather prevents the return of that fluid into the glaſs (which had before been thrown out into the finger) which would have produced the ſparks.

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* N. B. When at any time we mention the electric fluid as being condenſed in the glaſs itſelf, I would be underſtood that ſuch condenſation may be made *at* or exceedingly *near* the ſurface; but, ſuppoſing the latter, whether near the inſide or outſide thereof, I cannot yet ſay. However, I do not deſpair of making ſome experiments which

It ſeems therefore that the force of the machine was the cauſe of the condenſation of the fluid within the glaſs near its upper ſurface, notwithſtanding the reſiſtance at that ſurface; and that this condenſed fluid was the cauſe of that at the under ſurface dilating itſelf and paſſing out into the finger, notwithſtanding the reſiſtance at the under ſurface.

Now if on withdrawing the wire of communication, this condenſed fluid could dilate itſelf and paſs out back again at the upper ſurface, the fluid would likewiſe return from the finger into the glaſs at the upper ſurface and a ſpark would appear: but the reſiſtance at the upper ſurface, ſix, prevents this eſcape of the condenſed fluid out of the glaſs, as it endeavours to dilate itſelf only with the force, three.

Conſequently, the condenſed fluid within the glaſs near its upper ſurface is the power, remaining in the glaſs, when the wire of communication was withdrawn, that prevents the return of the ſparks.

Now in the experiment of the exhauſtead tube, the excited tube (having not ſufficient power to condenſe

which will determine what the fact really is. I am the more particular in this part, as Dr. Franklin's notion differs conſiderably from this, as may be ſeen in his letters, page 72 to 75, for he ſuppoſes a kind of partition in the center of the glaſs, which being of a cloſer nature than any other part thereof, ſuffers the electric fluid entering at one ſurface, to paſs no farther than to half the thickneſs of the glaſs, and is there condenſed.

any fluid within the ſubſtance of the glaſs of the exhauſted tube) muſt, when it is withdrawn, take away with it all the excited fluid it brought with it, ſtanding as an atmoſphere about it: and therefore the lucid appearance returned, and this is the reaſon of the difference between theſe two experiments.

Upon the whole, we know by experiment that the pane of glaſs in theſe circumſtances remains electrified; and we have all the reaſon in the world from the equal appearence of the ſparks, to believe that as much fluid paſſes out at one cover as paſſes in at the other; and therefore that the electrification of the pane of glaſs does not ariſe from its whole quantity being either encreaſed or diminiſhed, as it does in metals, and other bodies.

There remains therefore but one way of explaining whence this electrification ariſes, and that is, the continuation of the condenſed fluid in the ſame ſituation at the upper ſurface, after the wire was withdrawn, and its continuing to act with the ſame force, as it did when the machine was in action; and therefore that the fluid muſt remain denſer near the upper ſurface, and rarer near the under ſurface of the glaſs, than it does naturally in unelectrified glaſs: or to return to our former manner of expreſſion, that the upper ſurface of the glaſs is electrified *plus*, and the under ſurface *minus*.

There is ſomething ſimilar to this ſtate of the fluid, in the experiment of the three bars in contact with each other, when the excited tube was applied at a certain diſtance from the middle of the middlemoſt bar: For whilſt the excited tube continues there, the middle bar is electrified *minus*, the others, *plus*. May not the three bars, as they are in contact with each other, be looked on as one bar? and will not the fluid ſtand denſer in the outer bars, and rarer in the middle one, than it did naturally, all the while the excited tube continues its action? So far then the diſpoſition of the fluid in theſe bars, conſidered as one body, is ſimilar to that of the fluid in the pane of glaſs.

But when the excited tube is withdrawn, the fluid immediately returns to its natural degree of denſity in all the three bars: becauſe there is no remaining force to reſiſt its doing ſo.

In the pane of glaſs, this return of the fluid to its natural degree of denſity within the whole glaſs, is prevented by ſome reſiſtance remaining at each of the ſurfaces after the machine is withdrawn, and acting with ſufficient force to ballance the endeavour of the fluid to do ſo. So long therefore as this reſiſtance continues, ſo long muſt the fluid continue in this unnatural ſtate.

* Now let us again compare (but more particularly) this unnatural ſtate of the fluid, in the pane of glaſs, with the experiment of the exhauſted tube, when expoſed to

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* The reader will pleaſe to obſerve, that in the other edition, the reaſoning upon the electric atmoſpheres, is not altogether agreeable to experiment. I ſay, *not altogether*, becauſe though there are really ſuch electric amoſpheres extended over each ſurface of the glaſs, yet they are neither ſo powerful nor ſo extenſive, as they are there ſuppoſed to be. I have therefore choſe to vary and illuſtrate that part, which relates to the experiment, by a farther analogy between the exhauſted tube and the pane of glaſs, as it appears to be the cleareſt and moſt ſatisfactory method. And I do think if my worthy friend had now been living, he would readily have concurred in this alteration.

the excited one, which we have more than once mentioned before in this work, and we ſhall imagine have all the viſible proof of their being in the ſame ſtate that an experimental analogy is capable of affording us.

And firſt, I conſider that when the excited tube is held at a certain diſtance from the exhauſted tube, and where it exerts its full force againſt the fluid within, next the inner ſurface of the ſame; it is exactly analogous to that ſide of the pane of glaſs electrified *plus*, exerting its full force againſt the fluid on the oppoſite ſide thereof. In the firſt caſe the greateſt quantity of light is ſeen to dart away through the vacuum without any return. And in the ſecond caſe the oppoſite ſide is electrified *minus* without any return.

And farther, ſo long as the tube is held at the ſame diſtance from the exhauſted tube, juſt ſo long the light ceaſes to be ſeen.

In the other, ſo long as the *plus* ſide continues in the ſame electrified ſtate, juſt ſo long does the *minus* ſide continue electrified in the ſame degree.

Secondly, Weaken the force in the firſt caſe, by withdrawing the excited tube from the exhauſted one, and a proportionable quantity of light returns.

Weaken the *plus* ſide of the pane of glaſs, by drawing away a little of the fluid with a pointed body, and the *minus* ſide will be weakened in the ſame proportion.

Thirdly, Increaſe the diſtance of the tubes from each other, and more light will return.

Draw more of the fluid from out of the *plus* ſide, and the *minus* will be weakened accordingly.

Fourthly, Withdraw the excited tube intirely from the other, and the remaining quantity of light will return, but after that there will be no more ſuch appearance.

Draw of all the *plus* electricity, and there will remain no ſign of a *minus* electricity on the other ſide.

Fifthly, the ſame lucid appearances happen as you approach the exhauſted tube with the excited one at the ſame diſtances, excepting that the light inſtead of returning, is driven away at each approach. And in this reſpect, it perfectly agrees with the electrifying of the pain of glaſs; for as one ſide becomes electrified *plus*, the oppoſite ſide is (as the virtue is driven of) electrified *minus*.

Laſtly, Let the excited tube be brought very near the exhauſted tube, and then draw it away quickly; in this caſe the greateſt quantity of light appears to return.

So with the pane of glaſs, the greateſt effect is produced by diſcharging the fluid on the *plus* ſide as ſuddenly. And experience has taught us the readieſt method, which is done by opening a communication at once between the two ſurfaces: as we ſaw it done by the perſons holding his fingers in contact with the under cover, and then bringing a finger of his other hand as quickly as he can into contact with the other cover, which he cannot do ſo quickly, but a ſpark will appear, and the glaſs be unelectrified at once or very nearly ſo.

And that the exhauſted tube is in a *minus* ſtate of electrification, at leaſt in ſome part on the inſide thereof, when the light is driven away, and therefore in the ſame ſtate with the pane of glaſs, appears from the violent ſhock it occaſions in the arms, when both ends are touched at the ſame time with each hand, after the tube has been expoſed to a much ſtronger power of electrifying than the excited tube is capable of producing.

But now we are advanced ſo far with the Leyden experiment, let us purſue it farther, and try another way of forming a communication between theſe ſurfaces, ſo that the pane of glaſs ſhall be gradually and quietly unelectrified. If a wire be bent in a circular form, ſo that its two ends, when it is brought to the pane of glaſs, may reach within two or three inches of the covers. If its ends are tapered down into fine points, and the middle of it be faſtened to a piece of ſealing wax, and then brought by this ſealing wax held in the hand into the ſituation we have deſcribed, the glaſs will quietly and gradually return to its natural ſtate, the denſer fluid eſcaping ſilently through this wire into the rarer: and what is worth notice, the wire afterwards will not be electrified, notwithſtanding it is ſupported by wax, which confines the fluid within it more than a perſon's hand would; and notwithſtanding that when the experiment is made in the dark, a very viſible ſtream of light appears at each end of it, till the glaſs is unelectrified. May we not therefore conclude, that nothing more is done in this experiment, than letting the equilibrium be gradually reſtored within the glaſs? May we not likewiſe conclude, that the overplus of fluid in the condenſed part was exactly equal to the deficiency in the dilated part;

for otherwiſe the wire would have been electrified either *plus* or *minus*? and therefore, that during the electrification of the glaſs, as much of the fluid was thrown out of the glaſs, as was thrown in; which only ſeemed probable before this experiment was tried. May we not likewiſe conclude, that nothing more was done in the firſt way of diſcharging the glaſs, than in this, only it was obliged to be done at once; and the violence was owing only to the encreaſed velocity with which the fluid was forced to move into the finger at the upper ſurface? And that this encreaſe of velocity was ſo great, that what in one caſe was only a continued ſtream of faint bluiſh light, was in the other kindled at once into a ſtrong ſpark of fire?

This may ſeem a very minute way of conſidering the cauſes of theſe appearances, but it ſeems to ariſe from the appearances, and to be confirmed by the following farther obſervations on the ſame ſubject.

The firſt obſervation is, that when the wire of communication with the machine is brought into contact with the upper cover, and the under cover is left expoſed every where to the air; and the machine is put into action, it will never be able to electrify the pane of glaſs, though its force be ſuppoſed ever ſo great, becauſe no fluid can be thrown out of the under ſurface, on account of the joint reſiſtances of the glaſs, the cover, and the air ſurrounding it.

But it may be urged, that this joint reſiſtance muſt be limited, and conſequently a power in the machine may be imagined greater, and conſequently ſufficient to electrify the pane of glaſs. To this I anſwer, that no more of the force of the machine can act upon the upper cover, than what it can communicate to the metal wire, and that all bodies can be electrified but to a certain degree, and conſequently, the force of the machine on that upper cover is limited and but ſmall in compariſon to the joint reſiſtance abovementioned.

The force of the machine therefore need only be ſtrong enough to electrify this wire as much as it will admit of; all ſuperior force, by irregularly electrifying the air about the wire, being of diſſervice to the experiment.

The ſecond obſervation is, that if a bent wire (as deſcribed above) is thus applied at its two ends in contact, (or nearly ſo) with the two coverings, before the machine is put into action, the machine will never be able to electrify the pane of glaſs; notwithſtanding the wire of communication with the machine, and the finger be each in cotact with the upper and under cover; becauſe the electrical fluid, conducted to the upper cover by the wire, has a freer paſſage through the bent wire to the under cover, than to the pane of glaſs, as metal reſſſts leſs than glaſs.

This is a father confirmation of the cauſe alledged for the bent wire's diſcharging the glaſs, when it was electrified; for ſurely the ſame cauſe that prevents an effect from being produced, is able to deſtroy that effect, when it is produced.

The third obſervation is this, that without ſuch a wire to form ſuch a communication between the two ſurfaces,

faces, the electric fluid on the plus ſide will of itſelf ſtream out, and ſpread over to the other ſide, whenever the covers of metal reach too near to the edges and corners of the pane of glaſs; the electrical fluid inſinuating itſelf between the air and the glaſs, till it reaches either the corner and edges of the glaſs, or of the under covering; and ſo preventing the electrifying of the glaſs at all, or very ſoon unelectrifying it, if it is electrified to any degree.

This is the reaſon why I gave a particular caution, that two, or three inches at leaſt of the under and upper ſurface of the pane of glaſs ſhould be left uncovered.

The next thing obſervable is, the great reſemblance there is in theſe two ſurfaces of the pane of glaſs, electrified one *plus* and the other *minus*, with the two cork balls, when one was electrified *plus*, and the other *minus*.

In order to ſhew this, ſuppoſe two panes of glaſs, ſupported each at the corners by four drinking glaſſes, to be electrified equally in the manner we have ſeen; and afterwards to be left to themſelves.

When they are thus left to themſelves, 1°. Let a wire form a communication between the two upper ſurfaces.

No alteration will happen, becauſe the two ſurfaces, thus communicating together, act with no greater force at each upper covering, than they did before they communicated together.

Thus, when two balls are brought together, after they are each electrified *plus*, they do not unelectrify each other. They only repel each other; and ſo would the

panes

panes of glaſs, if their repulſive force were ſtrong enough, to do it as they are now ſituated.

2°. Remove the wire, and let it now form a communication between the two under covers.

No alteration happens, and for the ſame reaſon. Thus when two balls were brought together, both electrified *minus*, they did not unelectrify each other.

3°. But if with two ſuch wires, a croſs communication be made from the upper cover of the firſt pane to the under covering of the ſecond, with one wire; and with the other from the upper covering of the ſecond to the under covering of the firſt: the condenſed fluid will no longer be confined, but a free paſſage will be opened to delate into the panes of glaſs at thoſe parts, where they are electrified *minus*, and will circulate round, and reduce itſelf to its natural ſtate both in the panes of glaſs, and in the wires, and not one of them will afterwards remain electrified either *plus* or *minus*.

In the ſame manner two balls, when one is electrified *plus*, and the other *minus*, and they are brought near one another, run together and unelectrify each other immediately.

It would be endleſs to produce all the experiments, that could be brought in confirmation of this way of accounting for the uncommon appearances, that occur in thus electrifying and unelectrifying glaſs: which agrees ſo exactly with all the other experiments we have produced; or rather, which takes its riſe from them.

I ſhall therefore ſtop here, and only deſire the reader not to carry this compariſon of the effects on the different ſurfaces of the electrified pane of glaſs, with thoſe on the cork balls, electrified *plus* and *minus*, too far. I mean only to compare them in general as ſimilar effects; and do not here conſider the comparative ſtrength of the powers producing theſe ſimilar effects.—Let us now return to the particular ſubject of this enquiry.

Thus have we ſeen that the variety in the reſiſtances made by different bodies to being electrified, has led us into the knowledge why bodies act ſo differently in one ſituation from what they do in another, according to the nature and quantity of the bodies they are contiguous to; and enabled us to explain the moſt amazing appearance of all, that of the *Leyden* bottle, which has excited the attention of all the philoſophers (I may almoſt ſay) in the known world.*

This our ſucceſs in purſuing this train of reaſoning is ſurely ſufficient to encourage us to enquire farther, where this reſiſtance is exerted, and from what power within the body it ariſes.

Now from our knowing by experiment that every body more or leſs reſiſts being electrified; which is now allowed to mean, that every body *reſiſts* the paſſage of the electrical fluid either into them, or out of them, though

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* N. B. The Leyden experiment is not confined to glaſs only, for it ſucceeds with other ſubſtances, though perhaps, not ſo well; this hath been obſerved by ſeveral gentlemen. And it is very remarkable, that the experiment will not ſucceed with ſome particular ſubſtances, *unleſs they are heated to a certain moderate degree*; *but why heat ſhould make this difference*, and in certain *bodies only*, is another inquiry. The account of theſe laſt curious facts, was ſent me in a letter along with ſome other experiments, by Edward Delaval, Eſq; which I communicated to the Royal Society in March laſt.

*ſome* do it with a *greater* force than *others*; we may very reaſonably conclude, that when this fluid is endeavouring to get into any body, the reſiſtance it meets with is exerted at that particular ſurface, at which the attack is made: and when this fluid is endeavouring to get out of any body, the reſiſtance it meets with is exerted at all its internal ſurfaces at once; and, if the force with which this fluid endeavours to get in or out, is ſuperior to the force, which endeavours to keep it out or in, at any place or phyſical point of any of its ſurfaces, it muſt make its way more eaſily there, and conſequently will always paſs that way either in or out.

And there is an appearance in glaſs that is exactly analogous to this, with regard to the reflection of the rays of light, by which it is evident, that a pane of glaſs reſiſts the entrance of light in at one ſurface; and when it has got in, it as ſtrongly reſiſts its going out at the oppoſite ſurface; becauſe at both theſe ſurfaces, it reflects or drives off great numbers of rays, that without ſuch reſiſtance would have paſſed readily through the pane of glaſs.

It is likewiſe known, that the rays reflected thus back into the glaſs, upon their endeavour to get out, and returned again to the firſt ſurface, through which they had once got, meet again with an equal reſiſtance there to their paſſing out, and are many of them reflected into the glaſs again.

It is ſhewn farther by Sir *Iſaac Newton*, that this force begins to exert itſelf on the rays of light before they arrive into contact with either of theſe ſurfaces.

And hence we have reaſon to believe, that the reſiſtance we have experimentally ſhewn to be made by all bodies againſt being electrified either *plus* or *minus*, is of the ſame nature with that, which not only prevents the entrance and exit of the rays of light in glaſs, but throws them off with the ſame velocity, great as it is, with which they endeavour to get into it, or out of it: and therefore moſt probably ariſes from the ſame cauſe.

And conſequently, that this reſiſtance in bodies to being electrified is exerted, as the other is, before the electrical fluid comes actually into contact with the ſurface of any body it endeavours to electrify.

The experiments therefore in Sir *Iſaac's Optics* ſerve greatly to confirm the opinion, we have endeavoured to eſtabliſh by electrical experiments, that every body reſiſts the entrance and exit of this electric fluid; and that this reſiſtance is exerted at ſome diſtance, before the fluid, that endeavours either to get in or out, arrives at the ſurface, where the endeavour is made.

Our next enquiry is, whence this power ariſes, that is thus exerted at the ſurfaces of bodies.

In order to anſwer this enquiry, I ſhall firſt propoſe the examination of the following experiment, agreeably to the way of arguing we have hitherto made uſe of.

Let a perſon ſtanding on wax electrify a tube, and let another perſon ſtanding on the ground take as many ſnaps, as he can from the tube. There will ſoon be a time, when the perſon on the ground will not be able to get any more ſnaps.

But if, when this happens, the perſon on the wax ſets one of his feet on the ground, and keeps it there; the other on the ground may take ſnaps from morning to night, if he pleaſes.

From which plain experiment, the following conſequences may very evidently be drawn.

1°. That there is a quantity of this electrical fluid naturally in the perſon, ſtanding on the wax, that by the contrivance in making the experiment is thrown into the perſon, ſtanding on the ground.

2°. That there is but a certain limited quantity of this fluid in the perſon, ſtanding on the wax. Becauſe the ceſſation of the ſparks ſhews, that there remains no more of this fluid, that at firſt produced them: or at leaſt, that if there does remain ſome in the perſon, it has not force enough to puſh out, and paſs into the other, that ſtands on the ground, and endeavours to take a ſnap, as he did before.

3°. Therefore the cauſe of the fluid's paſſing out, and producing the ſnap, is the condenſation of it at that part of the perſon, which touches the tube in rubbing it; and ſo long as this condenſation of it can be made with a degree of force that is ſuperior to the reſiſtance made againſt its eſcape, it will continue to produce ſnaps: but no longer.

4°. Now the perſon ſtanding on the ground muſt be conſidered as one body with the earth, and what little fluid is thrown into him cannot produce any ſenſible encreaſe of denſity in the immenſe quantity on the whole ſurface of the earth: and therefore he will not be ſenſibly electrified, how many ſnaps ſoever he may have taken.

5°. When the perſon on wax does but touch the ground with his foot, he likewiſe muſt then be conſidered as one body with the earth, and conſequently, that the degree of denſity in his whole body cannot be ſenſibly altered, though ever ſo many ſnaps are taken; becauſe whatever paſſes out of him, paſſes into the other, *i. e.* in their preſent circumſtances, paſſes into their common ſtock in the earth.

6°. In theſe circumſtances therefore neither of theſe perſons ought to continue to be electrified, according to our reaſoning: and it is true in fact that they do not.

7°. Whence it ſeems to follow that every animal that ſtands on the earth, every vegetable that grows on it, the whole watery element that flows upon its ſurface, all minerals, and metals within the earth, as well as the air without, partakes of this common ſtock in the general courſe of nature: without being ſenſibly electrified.

Whence therefore ariſes their reſiſtance to being *ſenſibly* electrified? In order to anſwer this; we have ſeen, that whilſt any body continues to be ſenſibly, or perceptibly electrified, whether it be *plus* or *minus*, in any of the experiments made on them by art, there is an atmoſphere of the ſame kind of fluid which naturally belongs to, and is formed round them, ſufficiently ſtrong to ballance every power, that endeavours to electrify them above a certain degree: otherwiſe they might be electrified more and more *without limit*.

This atmoſphere therefore, thus ſurrounding the ſurfaces of bodies, when artificial force electrifies them, is what reſiſts their being electrified *more*; and when it abſolutely prevents it, muſt be equally denſe and powerful with that electrical fluid that flows from the excited tube, or machine, which endeavours to force its way through theſe atmoſpheres into the bodies, in order to electrify them *more*.

In the ordinary and quiet manner, in which the imperceptible works of the Author of nature are carried on among the component particles of the groſs bodies on or near the ſurface of the earth; this ſubtile, active, electrical fluid, which not only ſurrounds each groſs body, but pervading its pores, ſurrounds every component particle of it, where it is not in abſolute contact with its neighbouring component particle; this active fluid, I ſay, cannot be idle, but muſt be in action, though that action be imperceptible to our ſenſes: and it muſt in an imperceptible degree be varying its condition in all the parts of bodies whatever, *i. e.* in our preſent way of expreſſing of ourſelves, be electrifying them *plus* or *minus*, though not ſo forcibly, as to give ſenſible ſigns of it.

We may therefore not unreaſonably conclude, that all bodies whatever in their natural ſituation, and all their component particles, have ſurrounding their ſurfaces, not in abſolute contact with other ſurfaces, an imperceptible atmoſphere, of elaſtic particles, ſufficient to ballance the ſmaller force with which they are attacked; every way ſimilar to the perceptible atmoſpheres at the ſurfaces of bodies electrified forcibly either by art, or the violent exploſions in nature.

In theſe atmoſpheres, which ſurround the ſurfaces of all bodies, is placed the power, which occaſions the reſiſtance found experimentally to be made againſt thoſe bodies being electrified to a higher degree, than they are naturally before we attempt to electrify them perceptibly to our ſenſes: and the power is the elaſticity of this ſubtile electrical fluid, every where diſperſed indeed where groſs bodies are out of the way, but likewiſe confined within bodies differently, according to their different ſituations and neighbourhood to other bodies.

And theſe atmoſpheres may be encreaſed, or diminiſhed to a certain degree many ways by art; and when this is done with violence, the natural contexture of the bodies is altered in proportion to the violence.

Thus we ſee even, by the ſmall force of our electrical machines, very manifeſt tokens of the electrified bodies not only parting with their natural ſhare of the electrical fluid, but of many of their component particles; which may be perceived by the ſmell they yield on being electrified, and the rays of light they throw out, which, mixing with the air, occaſions *real Fire*.

Theſe are proofs of the atmoſpheres being encreaſed, as they only happen on the ſudden diſſipation of them by art, after they were thus encreaſed. Before they were thus diſſipated, therefore, they kept the body, they encloſed, *compact* and *entire*; and if the force, which had encreaſed them, had gradually withdrawn itſelf, theſe atmoſpheres would gradually have returned to their natural degrees of denſity, and the bodies to their natural ſtate, both with regard to their component particles, and their natural ſhare of this fluid and of the light within them, which were all diſturbed and in action before.

With regard to the encreaſe of denſity in theſe atmoſpheres, what has been ſaid may be little eſteemed as ariſing from theory: but it muſt be remembered that this theory has been very carefully raiſed from experiments, and can only be deſtroyed by ſhewing ſome fallacy in our reaſoning, or ſome experiment in contradiction to it. When this happens, this theory muſt either give way, or be improved.

With regard to the diminiſhing the degree of denſity in theſe atmoſpheres, I ſhall produce an experiment or two, very well worth attending to, as I think they will amount to an experimental proof, the beſt proof, of the whole doctrine I have advanced in theſe ſheets.

Glaſs, we have obſerved, and ſeen in experiments, is the moſt difficultly electrified of any of the common bodies we try our experiments upon: and therefore has the moſt reſiſting atmoſphere on its ſurfaces, according to our way of reaſoning, by which it ſo ſtrongly reſiſts electrification, *i. e.* the denſeſt atmoſphere.

Metals reſiſt being electrified with much leſs force, and conſequently have, in the ſame way of arguing, atmoſpheres on their ſurfaces which reſiſt but weakly, *i. e.* not the denſeſt atmoſpheres.

Heat is known to rarify all bodies, both fluid or ſolid, and if a glaſs be heated to a certain degree, even below melting, it will conduct, as it is called, the electrical fluid, *i. e.* it will reſiſt its entrance into it, or its exit out of it, no more than metals do: and when melted, no more than water.

A ſubtile and elaſtic fluid moſt undoubtedly muſt be rarefied ſooner than ſo denſe a body as glaſs, and therefore it ſeems moſt probable that the reſiſting atmoſphere around its ſurfaces is rendered equally rare, or weak by the heat, with that of metals, and therefore reſiſts the paſſage of the electrical fluid through the glaſs no more than braſs or any other metal does.

And this ſeems to be confirmed by the glaſs's reſiſting this paſſage through it more and more as it gradually cools: till at laſt when it is quite cold, it reſiſts as forcibly as ever againſt any entrance of the electrical fluid into it.

Wax is a ſubſtance, which reſiſts electrification perhaps as much as glaſs, but certainly more than metals, whilſt its ſurface is every where ſmooth and poliſhed: but wax is very eaſily melted with a ſmall degree of heat; and ſo its atmoſpheres are very readily brought down to the ſame degree of rarity with thoſe of metals: and therefore reſiſts the paſſage of the electric fluid no more than gold or any other metal.*

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* In the other edition, there is an experiment or two with a ſulphur globe, which was taken from the works of Dr. Franklyn: But as the experiments were never repeated either by Doctor Hoadly or myſelf, and the

But to make it ſtill plainer that the reſiſtance to electrification is made at the ſurfaces of bodies, I will ſhew in the following very curious experiments, that the electrical fluid in paſſing from one body to another will always take that way, in which it meets with the feweſt ſurfaces to break through.

Let a *Leyden* bottle, that has a hook in its coating, be electrified and ſet down on glaſs, and left to itſelf. Let one end of a clean chain (ſuch a one as is commonly uſed for a jack, but rather lighter) be fixed on to the hook in the coating; and let a perſon graſp the coating of the bottle with one hand, and with the other hand bring the other end of the chain and his finger and thumb that holds it, at the ſame time into contact with the wire of the bottle. Here then are two ways offered for the electrical fluid to paſs from the wire to the coating, either through the perſon, or the chain: and if the links of the chain hang looſely on one another, it paſſes through the perſon, and he is ſhocked very nearly as much as if the chain was not there. But if the chain be ſtretched by any contrivance, ſo that its links are all in abſolute contact with each other, the fluid will paſs through the chain, and the perſon will feel no ſhock at all. And this will be the caſe, let the chain be ever ſo long.

Whence it follows, that the electric fluid does *not* always paſs from one body to another by the *ſhorteſt way*,

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the reaſoning upon them was liable to objections, and not neceſſary in the preſent caſe, I have choſe to leave that part out in this 2d edition.

but on the contrary it will go about, and paſs that way in which it meets with *leaſt* reſiſtance.*

Now if the chain alone forms the communication between the wire and coating of the bottle, and it is ſpread ſo on a table, that the links of it ſcarce ſeem to touch one another; there will not only be a ſpark ſeen on the approach of the end of the chain to the wire of the bottle: but a number of them will appear very viſibly, when the experiment is made in the dark, *viz.* at every place, where the links do not abſolutely touch one another. But when the chain is ſtretched tight enough to have every link in abſolute contact with its neighbouring links, there appears but one ſpark on bringing the end of the chain to the wire of the bottle; the whole chain then forming one continued metal.

By the appearance of theſe ſparks, or their non-appearance, we judge whether the electrical fluid paſſes through the chain or no.

Now in theſe experiments, we have ſeen that when the links of the chain were looſe, the perſon was ſhocked, and the fluid paſſed through him; and that it did not paſs likewiſe through the chain appears from the experiment, becauſe no ſuch ſparks appear between the links.

Whence we may fairly conclude, that the reſiſtance made to the paſſage of the electrical fluid through the chain, ariſes from the ſum of the reſiſtances at the different ſurfaces of the links, it was to break its way through, in paſſing through the chain. Becauſe when the links were forced into contact with each other, and ſo the whole chain was made one continued metal, this reſiſtance is entirely taken off, the perſon is not at all affected on the diſcharge of the bottle, nor do any ſparks appear between the links.

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* Doctor Watſon and Doctor Franklyn are therefore miſtaken in regard to the Leyden experiment, where they advance that the electric fluid always makes the *ſhorteſt circuit.*

Now if the experiment be repeated in the following manner, this conſequence will be ſtill plainer. Let one end of a wire, ever ſo long, be faſtened to the hook at the coating of the bottle, as well as the chain, and let a perſon, graſping the bottle, as before, bring the other ends of both the wire and the chain into contact with the wire of the bottle, and let the links of the chain lie looſe on the table; or let the chain not be ſtretched.

Here are plainly three ways, the electrical fluid may paſs on the diſcharge of the bottle, through the perſon, through the chain, or through the wire: and the event of the experiment ſhows that it does not paſs through the perſon, becauſe he feels no ſhock; and that it does not paſs through the chain, becauſe no ſparks are ſeen between the links; and therefore that it paſſes through the wire, where it meets with but one ſurface to break through; *viz.* the ſurface at the end that is brought to the wire of the bottle, the other end of it being ſuppoſed to be in contact with the hook at the coating of the bottle.

I ſhall obſerve, by the by, that no one, who had not made the experiment, would imagine with how much force the chain muſt be ſtretched before the experiment will anſwer, and the electrical fluid paſs through it without producing a ſpark at any of the links, *i. e.* before the links can be brought into abſolute contact with each other: which one would naturally think their weight alone would be ſufficient to do.

But it appears that their weight will not do this, but that ſome additional force, independent of themſelves, or their weight, is required to bring them into abſolute contact.

This, therefore, is a ſtrong confirmation of Sir *Iſaac Newton's* aſſertion, that when a convex glaſs is laid upon a plane glaſs, they do not abſolutely touch; but that they muſt be ſqueezed together with ſome force before they can be brought into abſolute contact, ſo as to produce the effects viſible in ſome of his niceſt experiments, which depend on their abſolute continuity.

From all theſe obſervations, and experiments, we may conclude, that the power which produces the reſiſtance we find in bodies to be electrified either *plus* or *minus*, is the elaſticity of theſe ſmall atmoſpheres of the electrical fluid, which are formed at all their ſurfaces by the actions between the particles of this fluid (both within and without bodies) and the component particles of the bodies: and therefore muſt be different at the ſurfaces of different bodies.

We ſhall in the next place endeavour to explain, how light bodies at conſiderable diſtances from an electrified body, are driven to and from that body.

We have ſeen, that on bringing an excited tube near the middle of a bar, is electrifies it *minus*, and conſequently, puts the electrical fluid within it into a ſtate of dilatation; whence we may conclude in general, that this fluid every way ſurrounding the tube in the air to a certain diſtance, is in the ſame manner put into a ſtate of dilatation; and in conſequence of this, that beyond that diſtance, it muſt be put into a ſtate of condenſation: or more properly, that this fluid is rarer, than it naturally is near the ſurface of the excited tube, and grows gradually denſer and denſer, till as that diſtance, to which the power of the tube extends, its return to its natural denſity again.

Whence it will follow, that any light body, that is within this diſtance, will be forced from the denſer to to the rarer part of this fluid ſurrounding thus the excited tube; and ſo ſeem to be attracted by it.

But as ſoon as this light body in paſſing thus from the denſer part of this fluid through the rarer, during its approach to the excited tube, becomes ſufficiently electrified to have an atmoſphere collected round it, of the ſame nature with that of the excited tube, it muſt, as has been ſhewn before, be driven back again from the tube: and that its being thus driven back again, is owing to its having acquired ſuch an atmoſphere, is evident from this, that whenever afterwards it comes near any body more eaſily electrified than itſelf, and communicating with the ground, its electrical atmoſphere is diſſipated;

pated; and it immediately returns to the tube again; for the ſame reaſon, as at firſt.

Thus have we gone through the moſt intereſting of the electrical experiments, and from the various appearances they afford, it appears, that the electrical fluid is as univerſal and powerful an agent at or near the ſurface of the earth, as that fluid, which Sir *Iſaac Newton* in his *Optics*, calls æther; that it is as ſubtile and elaſtic in its nature, as æther is; and as æther does, that it pervades the pores of all bodies whatever, that we are converſant with; is diſperſed through whatever vacuum it is in our power to produce by art; and from the natural phœnomena of Thunder, Lightning, *&c.* ſeems to be extended to very great diſtances in the air.

We ſhall make no ſcruple therefore now to affirm, that theſe two fluids are one and the ſame fluid; as it is much more philoſophical to do ſo, than to ſuppoſe two ſuch fluids, each of them equally capable of producing theſe effects, and equally preſent every where; which would be multiplying cauſes, where there is no manner of occaſion.

The word *electrical*, is of too confined a meaning to be a proper epithet for a fluid of ſo univerſal an activity as this is found at laſt to be, from the experiments we have been conſidering, becauſe it expreſſes its power but partially.

*Electricity* means no more than the power we give bodies by rubbing them, to attract and repel light bodies

that

that are near them, in the ſame manner as amber does, when it is rubbed. But this fluid not only makes light bodies, that are near an electrified body fly to and from that body, and ſo appear to be attracted and repelled: but it heats them by putting their component particles, and the particles of air and light within them, into a vibrating motion; and makes them throw out the rays of light that before lay hid, and part with their ſulphureous and volatile component particles, which, with the rays of light, on mixing with the air, burſt out into ſparks of real *culinary* fire, as the chemiſts expreſs themſelves; nay, more in paſſing through animals, it occaſions convulſions, tremors, pain, and death ſometimes: and in paſſing violently through leaf gold, held tight between two pieces of glaſs, makes a fuſion of the gold, or a ſeparation of its parts, ſo inſtantaneouſly, that no ſenſible heat remains either in the gold or glaſs, and they immediately after are ſo cloſely united, that Aqua Regia itſelf, has no effect upon the metal.*

It is likewiſe improper to call this fluid, *Fire.*

Air may juſt as properly be called ſound, as this fluid can be called fire. When ſound is produced, the particles of the air are put into ſo regular a motion as to convey ſuch ſenſations by means of the ear as raiſe the idea of ſound. But air is not therefore ſound. In the ſame manner, when a body has all its component particles thrown into ſuch agitations in the air, by the force and action of this fluid within it and without it, that it grows hot, and ſhines, and glows and conſumes away in ſmoak and flame, we ſay the body is on fire, or burns; but this fluid is not therefore *fire*: nor can it, without confounding our ideas, have that name given to it; nor indeed can *fire* be called a *Principle*, or *Element* in the Chemiſt's ſenſe of the word, any more than *ſound* can.

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* I have made a ſmall alteration in this part, becauſe upon making the experiment (which we had not done before, but only related what ſeemed to be the fact when Dr. Franklyn firſt made the experiment) it has not hitherto appeared that the gold is vetrified, or even driven into the pores of the glaſs, as was ſuppoſed by him; but I ſuſpect that the metal is driven ſo near into contact with the ſurface of the glaſs, by the violence of the exploſion, that it is held there by an exceeding great force, and ſuch a one as we have been endeavouring to ſhew is exerted at the ſurfaces of all bodies whatſoever.

Sir *Iſaac Newton*, at the end of the *Principia*, in the ſecond edition, *anno* 1713, deſcribes this fluid and its effects in the following words, and ſays expreſly, that it is the cauſe of the electricity.

" Adjicere jam liceret nonnulla de ſpiritu quodam ſubtiliſſimo corpora craſſa pervadente et in iisdem latente: cujus vi, et actionibus particulæ corporum ad *minimas diſtantias* ſe mutuo attrahunt, et contiguæ *factæ* cohærent: et corpora *electrica* agunt ad *diſtantias majores tam repellendo quam attrahendo corpuſcula vicina:* et lux emittitur, reflectitur, refringitur, inflectitur, et corpora calefacit: et ſenſatio omnis excitatur, et membra animalium ad voluntatem moventur vibrationibus ſcilicet hujus ſpiritus per ſolida nervorum capillamenta ab externis ſenſuum organis ad cerebrum, et a cerebro ad muſculos propagatis. Sed hæc paucis exponi non poſſunt; neque adeſt *ſufficiens copia experimentorum*, quibus leges actionum hujus ſpiritus accurate determinare et monſtrari debent."

No one, we think, can read this paragraph, after having conſidered the appearances in the experiments deſcribed above, without recollecting inſtances in ſome one or other of them, of almoſt all the effects of this fluid, enumerated in it: and agreeing with us, that the other appearances among electrified bodies, as well as that of their repelling and attracting light bodies that are near them, may all of them ariſe from the force and action of this fluid, on the component particles of the bodies; on the rays of light within them; and on the air they are in; and the reaction of theſe upon the æther.

When a flint and ſteel are ſtruck together with ſufficient force and veloſity, a ſpark of fire, as we call it, is produced, which readily fires gunpowder, or lights tinder: but ſoon cools, if left to itſelf.

Now if ſuch a ſpark be caught on a ſheet of paper, and examined in a microſcope, it will be found to be a piece either of the flint or of the ſteel, ſtruck off, ſo exactly ſpherical, and poliſhed, that the windows of the room may be ſeen in it in the ſame manner as they are in a large poliſhed ſphere of metal or glaſs: and they could not be ſo ſpherical, and well poliſhed, as they are found to be, if they had not been melted and kept in this form by the cohæſion of their component particles.

In either of theſe caſes, a piece of flint or ſteal is evidently ſeperated from the body, and its component particles put into ſuch agitations among each other, as to throw off the rays of light that were among them, and ſhine and melt, and afterwards cool in a ſpherical form: by the action of the æther on light and air, and theſe component particles; and the reaction of theſe upon the æther; on their being all put into action at once by the briſkneſs of the ſtroke.

There would have been no ſuch ſpark produced, if any of theſe had been wanting; and conſequently, they are all neceſſary, though perhaps not equally ſo, to the producing this effect; the æther ſeeming to be as powerful an agent as any amongſt them; without which the inteſtine motion among the component particles of the piece ſtruck off, could not have been kept regularly up, even for the very ſmall time in which theſe changes are made in that piece.

In the ſame manner are the appearences of light in theſe electrical experiments, whether in faint ſtreams of different colours, or in bright and active ſparks, to be conſidered; as riſing from ſmaller parts of groſs bodies ſeperated from them, and carried off by the activity of the excited æther, paſſing from one body into another; which parts, though imperceptible to us, muſt have their component particles put into agitations amongſt themſelves, and, in being decompoſed, part with the light (that before lay hid within them) and their moſt volatile particles; and ſo ſhine, and ſmell, and explode in paſſing through the air.

And not only theſe appearances of light, ſparks and exploſion, but the effects of them on bodies, expoſed to them in electrical experiments, ſeem all to be explicable by the mutual action and reacton of the æther, of the component particles of the ſmall parts of bodies thrown off in theſe experiments, of the particles of light within theſe, and of the air, one upon another, when they are once made active by friction.

A more minute, or exact explanation of every particular appearance of this kind in each electrical experiment, we were to conſider, was never deſigned in this enquiry; as has been ſaid before. Our intention being only to ſhew from a number of experiments, that whatever fluid was the cauſe of the very ſurpriſing effects produced in them, muſt be of the ſame nature, and as univerſal, and as powerful, as the æther which Sir *Iſaac* in his *Optics* ſuſpects even to be the cauſe of gravity.

Theſe experiments, therefore, ſeem to us ſo many confirmations of the exiſtence and properties of ſuch a ſubtile, elaſtic fluid every where diſperſed about the earth; and though they ſhould not be thought abſolutely to prove its exiſtence every where, they may be fairly added to the number of thoſe experiments, that cannot be ſatisfactorily explained without it: and by putting us in a right track, may perhaps enable us to obtain a ſufficient plenty of theſe ſort of experiments to make us certain there is ſuch a fluid actually exiſting every where; and what the laws of its action are.

If the laying theſe experiments in the order we have done, and our reaſoning upon them, ſhall any way conduce to ſo valuable an end; we ſhall think our time very well employed, and our purpoſe anſwered.

We are very ſenſible, that there remains to be explained, how rubbing a tube rarifies the æther around it; nay, more, how the tube once excited by rubbing will retain this virtue for a conſiderable time after the rubbing has ceaſed: but this we muſt leave to another opportunity, the explanation of it neceſſarily leading to greater lengths, than our preſent deſign ſeems to require.*

We ſhall only add, with regard to Sir *Iſaac Newton*, that this opinion of his was no new one, taken up in his latter days, in order to obviate the trifling objections of foreign philoſophers againſt the uſe he made of the words Attraction and Coheſion; but was the reſult of his experiments in *Optics*, the greateſt part of which he made long before the *Principia* was publiſhed: and in order to ſhew this, as well as the ſtrict adherence to the truth of appearances he obſerved in all his philoſophy; we ſhall conclude with a ſentence or two out of the Preface to the firſt edition of the *Principia*, printed in the year 1686: recommending them to the conſideration of every one, who is diſpoſed to a philoſophical enquiry into any part of the works of the great Author of all nature.

"Omnis enim Philoſophiæ difficultas in eo verſari videtur ut a phænomenis motuum inveſtigemus vires naturæ; deinde ab his viribus demonſtremus phænomena reliqua

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* See a Treatiſe of electricy, by B. Wilſon, where this enquiry has been attended to, page 99, prop. 16.

et huc ſpectant propoſitiones generales quas libro primo et ſecundo pertractavimus.

In libro autem tertio exemplum hujus rei propoſuimus per explicationem ſyſtematis mundani.

Ibi enim ex phænomenis cœleſtibus, per propoſitiones in libris prioribus mathematice demonſtratas, derivantur vires gravitatis, quibus corpora ad ſolem et planetas ſingulos tendunt. Deinde ex his viribus, per propoſitiones etiam mathematicas deducunter motus planetarum, cometarum, lunæ, et maris.

Utinam cætera naturæ phænomena ex principiis Nam *multa me movent ut nonnihil ſuſpicer ea omnia ex viribus quibuſdam pendere poſſe, quibus corporum particulæ per cauſas nondum cognitas, vel in ſe mutuo impelluntur et ſecundum figuras regulares cohærent, vel ab invicem fugantur et recedunt: quibus viribus ignotis, philoſophi hactenus naturam fruſtra tentârunt.*

Spero autem quod huic philoſophandi modo, vel veriori alicui, principia hic poſita lucem aliquam præbebunt.”

# *POSTSCRIPT.*

WE cannot help beſpeaking the reader's candour with regard to the difficulty of expreſſion on ſo nice a ſubject, as well as the difficulty of deſcribing with accuracy all the circumſtances in theſe nice experiments which are neceſſary to the production of the appearances we argue from; and muſt take upon us at the ſame time to aſſure beginners, or thoſe not very converſant in making theſe experiments, that they muſt not conclude they are not true, becauſe they cannot make them ſucceed.

The many and nice precautions in making all ſorts of electrical experiments with accuracy, may be ſeen very carefully enumerated at the beginning of Mr *Wilſon*'s treatiſe on Electricity, and to the ſame treatiſe we refer for the experiments, from which we deduce the circumſtances in which bodies differ in their degrees of reſiſtance to being electrified and unelectrified; mentioned page 3 and 4 of theſe *Obſervations.*

## To Dr. HOADLEY.

30 *Nov.* 1755.

THE honour you have done me, in deſiring a brief account of the moſt material diſcoveries I have at any time made in electricity, demands my immediate compliance.

I began my experiments and obſervations in the year 1745, and in the ſame year preſented part of them to the Royal Society. What my notions were then, you will find in thoſe papers which were afterwards publiſhed; the ſubſtance of which, however, I will here ſet down.

That what we call electricity or electric matter, is a fluid exceedingly ſubtile and elaſtic.

That it readily pervades all bodies, and is diffuſed every where; ſo that the earth itſelf, as well as the air, is filled with it; but in ſuch manner, that its denſity may be encreaſed or leſſened by different degrees of heat and other cauſes, as the air is.

That any one body under certain circumſtances, cannot be electrified without *putting more* of that matter into it, than originally belonged to it. And ſo much of the electric matter as that body hath *gained*, muſt be taken away from the original quantity of ſome other body or bodies near it, or from the air, earth, or all of them together.

THE following letter to Dr. Hoadly was written at his requeſt, to give him my notions upon electricity, and of all that I had done therein. It is a kind of hiſtory of ſeveral appearances I have at various times obſerved in the courſe of my electric enquiries, and in ſome reſpects is nearly the ſame with the matter contained in the preceding work. Yet as it is not only ſhorter, but requires leſs attention, in the reading, to underſtand, I have been adviſed to publiſh it, and at the ſame time ſubjoin two other letters which were never before publiſhed, and are there referred to, as they chiefly relate to a particular fact that I obſerved in the year 1746, concerning the electric fluid as collected (in a great meaſure) from the earth itſelf.

And on the contrary any one body under certain other circumſtances, cannot be electrified without *taking away* or *leſſening* that quantity of electric matter which originally belonged to it. And ſo much of the electric matter as that body hath loſt, muſt be gained by ſome other body or bodies near it, or by the air, or earth, or them all together.

That the electric effects, ſuch as the attraction, repulſion, ſenſation, &c. ariſe from this loſs or gain, and are always in proportion; that is to ſay, the more any body has gained, the greater are the effects on its ruſhing out again, in order to reſtore its equilibrium. And *vice verſa*, the more any body hath loſt, the greater are the effects on its ruſhing in again, to reſtore the equilibrium.

The air which is an elaſtic fluid is governed by this law. I was of opinion, therefore, that from this law, all the electric phænomena, or moſt of them, might be explained.

But an elaſtic fluid of any given denſity, being actually detained within a bar of iron for any given time, without ſomething to *confine* it there, was a very difficult problem with me. So likewiſe was the ſingular property of glaſs, wax, &c. compared with iron and other metals, &c. the firſt obſtructing the paſſage of the electric fluid through them into other bodies, and the laſt ſuffering it to paſs moſt freely and readily through them into other bodies of the like kind. My difficulties continued for ſometime. But new experiments (which I publiſhed in the year 1746) and Sir Iſaac Newton's opticks induced me to believe.

That *all bodies have a very denſe ſubtile medium ſpread over their ſurfaces, which extends to a very ſmall diſtance from the body:* and is of the ſame nature with what is called electric fluid.

That the difference between electrics and non electrics, is intirely owing to the different denſities of this medium, as it reſiſts the exit or entrance of the electric fluid equally; ſo that bodies cannot be electrified plus, without this reſiſting medium to confine the electric fluid in the bodies when once they are electrified; no more than bodies can be electrified *minus* without the ſame reſiſting medium to hinder the electric fluid entering into the bodies.

Upon theſe principles I proceeded to make electric bodies non electric; or in other words, to render all bodies capable of receiving the electric fluid equally. And here I hop'd to diſpell as it were the cloud with which this part of natural philoſophy had for ſo many years paſt been invellop'd. I conſidered in the firſt place that if I rarified the air, or my other elaſtic fluid that was not confind, the reſiſtance ariſing from ſuch a diminution of denſity would be leſs than if it was not rarified. And therefore, if I expoſed amber, glaſs, wax, pitch, reſin, and even diamonds to any given degree of heat, I ſhould rariſy the medium upon the ſurface of any one of thoſe bodies, and conſequently leſſen its power of reſiſtance at any given diſtance. The event proved the truth of the conjecture, becauſe they were all in ſuch circumſtances as readily electrified as iron or any other metal.

Having ſatisfied myſelf that there really was ſomething upon the ſurfaces of bodies which actually had a power of reſiſting both the exit and entrance of the electric fluid, it was natural to conclude, that any two ſurfaces, or mediums belonging to the ſame body, would reſiſt more then one ſurface or medium of the ſame body, and three more then two, and ſo on. I therefore made the Leyden experiment with a chain; and conſidered each link thereof as having two ſurfaces at leaſt: ſo that lengthning or ſhortning the chain in each experiment occaſioned different reſiſtances; and the effects proved accordingly. To confirm this law of reſiſtance I made the experiment with one wire only, the ſurfaces of which were no more then two; and the effect anſwered my expectations, as the reſiſtance in that caſe was leaſt of all. But to leave no room for any doubt I cauſed the chain to be ſtretched, that the links might be brought nearer into contact, the event then was the ſame as with the wire. And this laſt experiment I looked upon as an *experimentum crucis*.

I was the more induced to proſecute this doctrine of reſiſtances, from what Sir *Iſaac Newton* has delivered in his opticks, concerning the priſms and convex glaſſes, where he obſerves that light is more readily tranſmitted through thoſe parts, which are in cloſer contact by *preſſing*, than in the other parts where they are not ſo much preſſed.* The perfect analogy between theſe experiments greatly confirms the truth of the reaſoning.

I then made ſome farther experiment, to ſhew that what is called electric fluid is of a very heterogeneous kind, and that it was probable we ſhould be nearer the truth, if we underſtood it thus.

That there is a very groſs matter driven off from the ſurface of bodies by the violence and vigour of the action of the electric fluid, which when forced into the air, kindles into flame. But the pure and ſimple electric fluid, which produces the wonderful effects of attraction and repulſion in electricity, and every other principal phænomenon, excepting the luminous appearances, is far more ſubtile than even light itſelf.

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* There is a curious phænomenon, which I have obſerved in the Torricellian experiment, made by the Right Hon. Lord Charles Cavendiſh, and mentioned in the tranſactions, vol. 47, p. 370, which affords us a kind of ocular proof of the exiſtence of this reſiſting medium upon the ſurfaces of bodies; for during that experiment, the ſtreaming light which appears to flow from the quickſilver in one of the laſs columns, through the *vacuum* to the quickſilver in the other column, is of an uniform denſity; *excepting at that ſurface where it enters the quickſilver*, and there the light appears to be much brighter, for near one tenth of an inch, and conſequently denſer. Now this increaſe of denſity (I ſuppoſe) ariſes from the reſiſtance at the ſurface of the quickſilver, where it endeavours to enter, and from no other cauſe. And that it will continue to increaſe till the force ariſing from its denſity, be ſufficient to overcome that which is exerted againſt it at the ſurface of the quickſilver. If the experiment be reverſed, ſo that the flow of the electric fluid ſhall be from the other column, then the ſame appearances happen, with this difference, that they are now upon the reverſe ſurfaces: and when either column is electrified *minus*, the appearances are the ſame.

From this its exceeding great ſubtilty and elaſtic power, I conjectured that it was diffuſed not only throughout the whole earth and air, but perhaps through the whole ſolar ſyſtem, becauſe we do not know of any confinement to limit its expanſive power; and experiments have taught us that the electric fluid always flows where it meets with the leaſt reſiſtance, whenever its *equilibrium* has been diſturbed, in order to recover its natural ſtate.

As this wonderfully active inſtrument attracted all kinds of bodies indifferently, I flattered myſelf that there ſeemed a perfect analogy between it and the gravitation of bodies. And that the reaſon why we were not able to produce greater effects, in moving larger bodies, by accumulating the electric fluid, was, that the ſurrounding medium not being ſtrong enough to detain the neceſſary quantity, our electric power was in ſome meaſure limited.

It was a conſiderable time after this, that Mr. Franklin, of Philadelphia, publiſhed his curious diſcoveries in regard to lightning, and the *plus* and *minus* electricity of a phial charged as in the Leyden experiment, in which he mentions a difficulty concerning the *minus* electricity which he could not account for, it was this. Why bodies electrified *minus* ſhould have electric atmoſpheres round them like thoſe electrified plus?

Here I perceived a new field of enquiry open before me, particularly as I believed the Leyden experiment could not be explained ſatisfactorily without a previous ſolution of this difficulty. Therefore after examining the ſubject a freſh, I drew up the firſt pacquet of papers you had from me, where you know I endeavoured to remove the difficulty we laboured under, both in regard to the Leyden experiment and the different repelling atmoſpheres of the *plus* and *minus* electricity. The method I proceeded upon to diſcover this *deſideratum*, was much of the ſame kind with that I made uſe of when I firſt found that the electric fluid did not come from the glaſs but the earth itſelf. For if (as I obſerved in a letter to Mr. Ellicot)* the glaſs or wax hinder the electric fluid from paſſing into the earth, they ought alſo to hinder its coming out again. So in this. If the medium ſurrounding a body electrified *plus* hinders the electric fluid from eſcaping out of that body, the ſame medium ought alſo to hinder the electric fluid from coming into a body electrified *minus*. Conſequently atmoſpheres round two bodies electrified *plus* and *minus* muſt neceſſarily be formed and be of equal denſities &c. provided the air ſurrounding the bodies be of the ſame drineſs and denſity, and the bodies of the ſame ſhape, kind and ſize.

I concluded my enquiries with giving a more general explication of the Leyden experiment, the particulars of which you received in my ſecond pacquet. So that I need not repeat in this letter the method I proceeded upon to ſolve this laſt and ſurpriſing problem. Neither need I tell you that Mr. Franklin's method was far from pleaſing me. I know you yourſelf was not ſatisfied with his reaſoning about it. All I have now to add is, that you will fulfil your promiſe and methodize thoſe papers for me. I care not how many objections are made by a good philoſopher, becauſe if I ſhould not be able to anſwer them, I ſhall perhaps gain ſomewhat from them that may contribute to improve my hypotheſis, and which I ſhould not have thought on had there been no objection made.

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* See the following Letters.

In the detail of this account there are a few things I have omitted, particularly the finding that the electric ſhock in the Leyden experiment was always proportional to the points of non electric contact on the in and outſide of the phial. But this and other experiments, however remarkable in themſelves, not being connected with my general ſyſtem, are therefore not of moment enough at this time to merit your attention.

I have omitted the references to ſave myſelf trouble, but if you deſire them they ſhall be ſent you.

I am, &c.

B. WILSON.

*An Extract of a letter to Mr. Ellicot, dated from Cheſter,* 17 *Sept.* 1746.

SIR,

I SHALL be very glad to know if you have tried the experiment I mentioned to you in London, concerning a large glaſs bottle filled with warm water, and touching the ſurface all over with a non-electric, in order to ſee if the effect is not in that proportion.

There is another experiment which I have ſince thought of, which I wiſh you would alſo try. I have ſuſpected that the excited effluvia are not from the glaſs globe but the body that rubs it, or however the greateſt part. You may be ſatisfied of this by holding in your hand a long dry bottle by the neck, ſo that the bottom thereof, to which one of your leather cuſhions is fixed, ſhall preſs upon the globe after the common method. In this ſituation you know the glaſs between the cuſhion and your hand will intercept any communication of theſe effluvia with the earth (if there naturally are any ſuch in non-electric bodies) for the ſame reaſon as it does electrified bodies when fixed upon reſin &c. I fancy *you will produce little effect.* If it ſhould be the ſame as before, then it is plain that the effluvia come from the globe of glaſs.

I am, &c.

B. WILSON.

## *An Extract of a Letter to Mr. Smeaton, dated from Dublin the 6th of Oct. 1746.*

DEAR FRIEND,

I SHALL defer giving you any account of this city, &c. till I write again, becauſe I have only time to acquaint you, that ſince my arrival in this kingdom, I have had an opportunity of trying ſome electrical experiments, which I had before thought of in England. And I have the pleaſure to inform you, that I find the electric effluvia do not come from the globe of glaſs, but from the earth itſelf, and all non-electric bodies, which are round about the apparatus. To try this, ſtand upon a board when placed on four bottles, or upon a cake of wax, and endeavour to excite a tube of glaſs; after that obſerve the effect. Then get off from the board or wax, and excite the tube again, and you will perceive a very great difference: for the effect produced in the laſt circumſtances, will be conſiderably greater than in the former circumſtances.

Another experiment which I propoſed to try, was with the *Leyden* bottle, whether it obſerved any law in the accumulation of electric matter. And I found that it was always in proportion to the thinneſs of the glaſs, the ſurface of the ſame, and that of the non-electric in contact with it on the in and outſide thereof. The experiment I made, was with water a little warmed, which was poured into the bottle, and the outſide was immerſed in a veſſel filled alſo with water, but a little colder, leaving three inches only, or thereabouts, uncovered, which was preſerved dry and free from duſt.

*I wrote to Mr. Folkes by the laſt pacquet, and informed him of theſe experiments, in order that he may lay them before that honourable ſociety, of which he is Preſident.*

I am, &c.

**B. WILSON.**

The letter to Mr. Folkes, was read before the Royal Society, Oct. 23, 1746, as appears by their minutes of that day, but the original being loſt or miſlaid, is, I ſuppoſe, the reaſon why it was never publiſhed.

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